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A LIVING PLESIOSAUR?: A CRITICAL ASSESSMENT OF THE DESCRIPTION OF *CADBOROSAURUS WILLSI*

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ABSTRACT: The biological interpretations made by Bousfield and LeBlond in their description of *Cadborosaurus willsi* (*Amphipacifica*, Vol. 1, Suppl. 1) are critiqued. There appears to be little support for their contention that the animal is a living plesiosaur. In particular, their reconstruction of locomotor morphology is inconsistent with that of all known plesiosaurs. We interpret the holotype of *C. willsi* to be a decomposed shark, and suggest that observations of "adult" *Cadborosaurus* may be based on large pinniped sightings. Cryptozoological specialization should be encouraged in order to ensure that investigations of unknown animals are conducted so as to meet the rigours of critical review.

INTRODUCTION

Cryptozoology as a science suffers from an image problem. Many critics (often uninformed) regard cryptozoology as the bailiwick of the fringe element, of credulous individuals with no real credentials as scientific researchers. Certainly, the field does attract a disproportionate number of adherents whose interests tend to the bizarre, and even to the supernatural. However, as a glance through the pages of the journal *Cryptozoology* will reveal, professional zoologists, anthropologists, linguists, and folklorists, as well as dedicated amateurs, are also intrigued by the field, and have established a credible record of objectively analyzing data pertinent to unknown animals (e.g., LeBlond and Collins 1987, Winn 1991), or applying data from other fields to cryptozoology (e.g., Buffetaut 1983, Brussard 1986).

Further, despite criticism to the contrary, cryptozoological research (either planned or serendipitous) has resulted in the discovery or rediscovery of a number of cryptozoans (see Raynal 1989 for a review of cryptozoological successes), and has even satisfactorily resolved questions of the origin of mythical beasts (Mayor 1991, Mayor and Heaney 1993). Perhaps the ultimate tribute to the dedication of cryptozoologists is that "mainline" peer-

reviewed scientific journals have been willing to publish research articles of a cryptozoological nature (e.g., Bodley 1988, Oren 1993, Pierce et al. 1995).

The most recent appearance of a cryptozoological paper in a scientific journal is the description of *Cadborosaurus willsi*, a large "sea serpent" from the Straits of Georgia, British Columbia, by Edward L. Bousfield and Paul H. LeBlond (Bousfield and LeBlond 1995). This was published in a new journal, *Amphipacifica*, that is ostensibly devoted to the systematics of invertebrates (especially amphipods) of the Pacific marine coastal region of North America. This would appear a rather strange place to publish a paper that describes a living plesiosaur, a view that was apparently shared by two of the three members of that journal's editorial board (the third member, the managing editor, being co-author Bousfield himself), who published a dissenting opinion in the same issue, justifying their opposition to publication of the paper (Staude and Lambert 1995).

Although we welcome another peer-reviewed outlet for systematic works (and for cryptozoological topics), we have major misgivings about the description of *Cadborosaurus willsi*, and believe the paper to be flawed both in premise and execution. We here present our objections, not as an attack on the authors, nor as a denial of the existence of a large, unknown marine animal species in the Straits of Georgia, but, rather, as a part of the necessary self-policing of cryptozoology as a discipline. Whereas most critics of cryptozoology seek to find poorly executed studies in order to debunk the discipline as a whole, we endeavor to provide as rigorous analysis of the data as possible, in an effort to salvage the data but discard interpretations that cannot be justified by these data.

Bousfield and LeBlond (1995) build their case on the basis of ethnological, testimonial, and zoological evidence. The latter is based primarily on photographs and interpretations of a supposed carcass of *Cadborosaurus* that was obtained from the stomach of a sperm whale flensed at Naden Harbour, in the Queen Charlotte Islands, British Columbia, in July, 1937. Also cited are observations of a captive juvenile. In analyzing the contribution by Bousfield and LeBlond (1995), we take as our premise the primacy of the zoological data in the case. That is, we will base our interpretations chiefly on the evidence from the Naden Harbour specimen, and what it does and does not (and can and cannot) tell us about *Cadborosaurus*. This approach differs from that of Bousfield and LeBlond (1995), who are credulous in their acceptance of all potential interpretations. By relying on the observational information as gospel truth, they place biology in a secondary position. They are forced by this approach to eliminate from consideration all interpretations that are not consistent with the eyewitness accounts, and thereby are led to biologically implausible or impossible conclusions. Any misinterpretations by witnesses are thus translated into inalterable truths, and the evidence from material evidence, common sense, and scientific reasoning are accepted or rejected in accordance with their compatibility with this gospel.

For example, they treat suggestions that whalers might not recognize parts of known animals as an affront to the whalers, despite the fact that Heuvelmans (1968) and others (Anonymous 1977) have shown that competent seafarers, as well as trained zoologists, have been confused by other such finds.

The ethnological evidence presented is largely irrelevant to the existence of *Cadborosaurus*. An atlatl pictured by Bousfield and LeBlond (1995) may or may not depict this animal, but the authors must consider alternative explanations. Such a device, because it is a functional entity and not merely an ornament, has a required size and shape, and, as in the spandrels of San Marco (Gould and Lewontin 1979), the designs used to decorate it are likely to be secondary to the function of the artifact. To insist that representations of unknown animals in artwork confirm the current or former existence of cryptozoans is to deny that mythology is a part of the culture, or, rather, that mythology must be based on physical reality. Although sometimes, as in the case of griffins in Greek mythology, there may be biological explanations for "fanciful" creatures (Mayor 1991, Mayor and Heaney 1993), in other cases allegorical or symbolic origins are clearly suggested. No biological models for the minotaur, for example, are likely to be forthcoming. We maintain that the authors cannot, without more extensive argument, accept some aspects of Indian legend and tradition as supportive of their contention that a plesiosaur survives in the Canadian Pacific, while at the same time dismiss other attributes (such as the ability to fly) that are inconvenient for their proposition. The long and consistent history of a "sea serpent" does not make it real. Only incontrovertible physical evidence can do that.

We have divided up our approach to the analysis of the case presented by Bousfield and LeBlond (1995) for *Cadborosaurus* into sections that deal with different aspects of the zoological evidence and its interpretation. Our evaluation does not cover every point raised in their paper, but goes into sufficient depth to indicate, we posit, that their evidence, as presented, falls far short of making a convincing case, both in terms of substance and critical interpretation.

NOMENCLATURAL ISSUES

Although non-specialists may regard the rules of nomenclature as the arcane regulations of a Victorian science, they are, in fact, the precepts by which systematists function. They provide guidelines for the maintenance of nomenclatural stability, and the recognition of legitimately described taxa. Although most modern systematists are interested in evolutionary or phylogenetic relationships, nomenclature and classification are the components of systematic study that most often interface with non-specialists. This, above all else, should argue for the significance of the rules of zoological nomenclature. To be sure, there is disagreement within the specialist com-

munity about details of the International Code of Zoological Nomenclature, and many aspects are likely to be modified in the fourth edition of the Code, due to be published in 1997. Indeed, an alternative code, based on phylogenetic principles, has been proposed (de Queiroz and Gauthier 1992, 1994), but even this would affect only certain aspects of the current Code, primarily at higher levels of hierarchical inclusiveness.

The description of *Cadborosaurus* is flawed by a series of very basic errors with respect to type designation, suggesting that the task was not taken very seriously. Most fundamentally, the nature of the type concept seems to have eluded the authors. A holotype is the single specimen designated as the name-bearing type of a species. Paratypes are specimens, other than the holotype, that constitute the type series. Bousfield and LeBlond (1995) have designated one photograph of the Naden Harbour specimen as the holotype of *Cadborosaurus willsi*, and another photo of the same specimen as a paratype. First, a photo or other illustration cannot serve as a type of a taxon. Article 72(c)(v) of the International Code of Zoological Nomenclature clearly states that "in the case of a nominal species-group taxon based on an illustration or description or on a bibliographic reference to an illustration or description, the specimen illustrated or described and not the illustration or the description" is the type (International Commission on Zoological Nomenclature 1985). Therefore, the holotype is the now lost or destroyed material which appears in the photographs. Second, only one individual organism was used in the description, so there is no paratype.

Bousfield and LeBlond (1995) purport to provide diagnoses of adult, juvenile, and newborn specimens of *Cadborosaurus willsi*. However, diagnoses are applied to species, not individual specimens, and their function is to differentiate the taxon under consideration from all others (in practice, this usually means from congeners or other closely-related forms), not to serve as a description. Descriptions (essentially, what the authors have presented as diagnoses) are typically based on the holotype, and may be supplemented by information from other type material. Some features in the "diagnosis" (=description) of the type, such as humps, are not seen or preserved in the type. In other respects, the "diagnoses" of the different age groups are in direct conflict. For example, there are no scales in the type, but the putative newborn is very scaly.

PHYLOGENETIC CONCERNS

A very basic flaw in the paper is that the authors apparently decided that *Cadborosaurus* was probably a plesiosaur, and then pressed this point, despite the fact that virtually all available evidence does not support this interpretation. Of course, *Cadborosaurus* might have a highly derived body form with respect to other plesiosaurs, but this would require unwarranted

assumptions, and would certainly not be a parsimonious interpretation of the data. (And to make such an assertion, one would expect that the authors would be able to claim considerable expertise in plesiosaur anatomy and systematics, which they certainly cannot.) By making this allocation, Bousfield and LeBlond (1995) have, of course, fixed the higher order placement of *Cadborosaurus*. Thus, they refer the species to the Class Reptilia (a paraphyletic group), and frequently refer to the "reptilian" features of their new species. Strictly speaking, because they are paraphyletic, "reptiles" cannot be diagnosed as a group, and thus have no unifying shared-derived features.

Setting this criticism aside, however, the authors provide no information of any general traits that would allow them to conclude that *Cadborosaurus* is reptilian. Indeed, features such as an ability to remain submerged indefinitely are clearly inconsistent with any air-breathing tetrapod. Likewise, the high surface area to volume ratio, combined with the cold temperatures of its presumed native waters, would not support the idea that *Cadborosaurus* has a typical "reptilian" metabolism. Significant heat production has been reported in one aquatic reptile, *Dermochelys coriacea*, but this ability is, at least in part, dependent on the low surface area to volume ratio of this large marine turtle (Frair, Ackman, and Mrosovsky 1972). On the other hand, elongate marine reptiles, such as the sea snake *Pelamis platurus*, are apparently unable to raise body temperature much above ambient water temperature (Dunson and Ehlert 1971), and are therefore limited to tropical waters.

Within the Reptilia, Bousfield and LeBlond (1995) further refer the species to the Euryapsida, now generally regarded as a polyphyletic group (Carroll 1988). This term is essentially meaningless, and its use suggests unfamiliarity with the modern literature. Dispensing with these issues, could there still be some validity in their allocation of the species to the Plesiosauria? How do the morphological features they discuss conform to the known features of plesiosaurs? In a word, poorly.

Among the features of *Cadborosaurus* are its elongate, attenuate body. This is not seen in any of the known plesiosaurs, which, in fact, have rather deep, barrel-like bodies with very robust girdles, entirely inconsistent with the slender body illustrated by Bousfield and LeBlond (1995). Further, the massive coracoid plate of plesiosaurs forms an inflexible shield spanning several vertebrae, and the abdomen is further reinforced by bony gastralria. Such a configuration would limit body flexion (even if the ludicrous vertebral angles proposed for *Cadborosaurus* were theoretically possible on the basis of their anatomy), and would preclude the body-looping described (see below).

Cadborosaurus, as reconstructed by Bousfield and LeBlond (1995), has a very short tail that is supposedly attached to the hind flippers by means of a fleshy membrane. Despite the obviously short "tail" in their illustrations, the authors, in their text, suggest a somewhat longer appendage, although it

is still quite short. All known plesiosaurs have tails of at least moderate length, and do not resemble the reconstruction in Fig. 13A of Bousfield and LeBlond (1995) in the slightest.

POST-CRANIAL OSTEOLOGY AND LOCOMOTION

The morphology of the locomotor apparatus is one of the most troubling aspects of the Bousfield and LeBlond (1995) interpretation. Bauer (1991) has previously cautioned against the reconstruction of improbable, if not impossible, morphologies in the attempt to reconcile physical evidence with testimonial accounts. In this case, the authors fully accept accounts claiming that the animal moves by throwing its body into tightly bent standing waves that are oriented vertically in the water. Muscles ripple on the animal, but the loops do not move. This almost unbelievably inefficient form of movement is achieved with the aid of a locking mechanism to hold the vertebrae in place, and is supposed to be a method of drag reduction.

Bousfield and LeBlond (1995) report that *Cadborosaurus* is capable of swimming speeds of up to 40 m.p.h. (64 k.p.h.). This is near the upper limits of aquatic locomotion, even for primary swimmers, including the fastest teleost fishes. However, the shape assumed by the body when near the surface induces large amounts of drag, increasing at these high speeds. Drag has several components: 1) friction drag; 2) pressure drag; 3) induced drag; and 4) wave drag. Shape changes affect friction drag and pressure drag differently, and a fineness ratio (body length/mean diameter of 4.5) generally yields minimal drag. Whales and ichthyosaurs, as well as some pliosaurs and plesiosaurs, fall close to this ideal (Massare 1988). Only short-necked plesiosaurs (e.g. *Cryptoclidus*, but not *Cadborosaurus*) would come close to this. Even for a "well-designed" 2.8 meter plesiosaur, a typical speed would have been 2.3 m/sec (8.28 km/h). Examining 18 specimens of 10 species, ranging in length from 2.49 to 12.56 meters (still short of the dimensions of *Cadborosaurus*), Massare (1988) calculated speeds of 2.17–2.51 m/sec. Alexander (1989) similarly estimated slow swimming speeds for this group, consistent with Robinson (1975), who postulated pliosaurs as sprinters, and plesiosaurs as slow endurance swimmers.

Bousfield and LeBlond (1995) claim that dorso-ventral flexion is a real possibility for plesiosaurs, or at least this plesiosaur, arguing from the evidence that Mesozoic crocodilians were probably capable of intervertebral excursion in this plane (Buffetaut 1983). However, it is a far cry from a moderate bow in the back to a series of sine waves along the trunk, especially since crocodilians and plesiosaurs are not closely allied. Vertebral number is fixed in any post-embryonic vertebrate. Thus, juveniles cannot add vertebrae, and could only change proportion by differentially changing the length of individual segments. As mentioned elsewhere, amniotes do not undergo metamorphosis, so any major restructuring is essentially ruled out. There-

fore, the morphology of the Naden Harbour "juvenile" should serve as model of the locomotor apparatus that any *Cadborosaurus* would have to work with.

From the photograph of this specimen, Bousfield and LeBlond (1995) estimate 26 dorsal vertebrae. Because "five or more" body loops, coils, or humps have been reported (and accepted by them), the animals must be able to throw the body into five loops with only 25 points of bending. This would require incredible amounts of excursion at each intervertebral joint. For contrast, Gasc (1974) found that angles of 10° were the approximate maximum between adjacent vertebrae in snakes. The vertical looping described by Bousfield and LeBlond (1995) occurs in no known vertebrates, the closest approximation perhaps occurring in a defensive posture assumed by certain small snakes, such as the Australian elapid *Vermicella annulata* (Shine 1980), although in this instance the pose is static and is accomplished by a body with many more vertebrae (between 200 and 300) than suggested for *Cadborosaurus*.

It is not necessary, however, to argue specific angles with the proposed mode of locomotion, since existing evidence from known plesiosaurs (not cited by Bousfield and LeBlond 1995) is clearly inconsistent with a highly flexible plesiosaur. In *Pachypleurosaurus*, Carroll and Gaskill (1985) reported 19–20 trunk vertebrae. These possess several accessory articulating structures that would "have made rotation of the vertebrae around the long axis nearly impossible, and might have also restricted lateral flexion [sic] of the trunk. . . . It appears to have formed an almost rigid structure in the living animal."

Indeed, although no primary literature on the subject was cited by Bousfield and LeBlond (1995), plesiosaur locomotion is well known and has been the subject of much recent research. Although differing in detail, there is a consensus among vertebrate paleontologists and functional morphologists that plesiosaur locomotion was oscillatory and limb-based, rather than undulatory and axial.

Both girdles form extensive plates of bone, and the limbs are subequal, enlarged, broadened paddles. The forelimbs were braced by massive coracoid bones (Nicholls and Russell 1991). Locomotion probably involved horizontal movement of the limbs in some combination of rowing and subaqueous flying (Robinson 1975, Taylor 1981, Tarsitano and Reiss 1982, Godfrey 1984, Carroll and Gaskill 1985, Massare 1988). Nothosaurs, with smaller limbs and longer tails than other plesiosaurs, may have paddled (Carroll and Gaskill 1985) or also used underwater flight (Sanz 1980). There were almost certainly asymmetrical power and recovery strokes (Godfrey 1984, Tarsitano and Reiss 1982, Frey and Reiss 1982), and locomotion may have been sea lion-like (Godfrey 1984, Taylor 1986).

Even the static appendicular morphology of plesiosaurs appears to have

been misrepresented by Bousfield and LeBlond (1995). Their Fig. 13C, for example, is problematic. It is supposed to represent the pelvic girdle of the plesiosaur *Cryptoclidus*, and to be derived from a popular book by Norman (1985). However, it matches exactly the pelvic girdle of *Pachypleurosaurus* illustrated in their Fig. 18, and taken from Carroll (1988: Fig. 12–4). Because this figure, as reproduced, is disassociated from the body, its orientation is incorrect, and it is this erroneous view that is applied as a model for the reconstruction.

The figure of *Cryptoclidus* in Norman (1985), however, is indeed the source of their Fig. 11. In the original illustration of the entire skeleton, the left foreflipper is seen in ventral view, as if it were raised at the end of the recovery stroke of the limb. However, in Bousfield and LeBlond (1995), the ventral view of the left flipper has been reconstructed as a dorsal view of a right foreflipper, and the digits are labeled incorrectly, with the fifth in the preaxial location, and the first situated postaxially. Thus the leading edge of digit one, as labeled, is actually the trailing edge of digit five.

The long neck, supposedly with 10 vertebrae (again based on the Naden Harbour specimen), has fewer vertebrae than have been recorded for plesiosaurs (Carroll and Gaskill 1985). The purpose of the long neck is supposedly to aid the animals in determining their position relative to the shore. This point is assailable on several fronts. If this is crucial to the animals, how do they orient at night? Second, it is likely that eyes that are “designed” for life in the mesopelagic zone may not be useful for distance vision in air. For example, sea turtles can accommodate in water, but have very poor vision in air (Underwood 1969). Thirdly, massive neck musculature would be required to support a huge neck and large head out of the water. In association with this, one would expect enlarged neural processes on the anterior trunk vertebrae for attachment of these muscles (or a stout ligament). These, according to the Naden Harbour photos, are not present in the type of *Cadborosaurus*.

RESPIRATION

The authors indicate that *Cadborosaurus* rarely comes to the surface, and can remain submerged indefinitely. This feature is not known in any amniotes. All amniotes require atmospheric oxygen while active over long periods. Although some may function anaerobically for short periods (Seymour 1982), the debt must be compensated for within a relatively short period of time. As counter examples, Bousfield and LeBlond (1995) present information about turtles that utilize buccopharyngeal linings as gas exchange surfaces, and are consequently able to remain submerged for long periods. While this does occur in some forms, as does cloacal respiration (Legler and Cann 1980, Seymour 1982, King and Heatwole 1994a), total non-pulmonary oxygen exchange capacity is still generally far below pul-

monary levels. In sea snakes, which use the integument as a gas exchange surface, the skin may account for up to 33 percent of total oxygen uptake (Heatwole and Seymour 1975), although the mean value approximates only 22 percent. The Bousfield and LeBlond (1995) claim that some euryapsids used non-pulmonary methods of respiration is both unknowable and indefensible.

Bousfield and LeBlond (1995) suggest that the large surface area to volume ratio characterizing *Cadborosaurus* would be beneficial, as it would provide a greater area for gas exchange. They fail to take into account, however, the fact that amniote skin is characterized by an outer keratinized investment that is largely impermeable to water and retards gas exchange (Lillywhite and Maderson 1982). The only known chordates to use the integument as the primary site of gas exchange are some juvenile fishes, cephalochordates, plethodontid salamanders, and a small number of other amphibians. All have thin, non-keratinized skin, and very small body size. Among amniotes, some sea snakes and turtles may use integumentary respiration as a supplemental means of gas exchange, but even this is limited (Seymour 1982).

The so-called mane reported in some eyewitness accounts of *Cadborosaurus* has also been cited to be a respiratory surface. This would only provide a limited amount of additional gas exchange surface. Further, such structures would of necessity be delicate, and their exposure to air and their subsequent clumping might cause damage. When in the water and traveling at 40 m.p.h. (64 k.m.p.), the strands would be adpressed to the body and/or to each other, increasing total surface area by a negligible amount. Further, these structures would need to be highly vascularized if they were to function in respiration (King and Heatwole 1994b), and they would provide a likely spot for the loss of metabolically produced body heat, a fatal “adaptation” in the cold waters of British Columbia. Among tetrapods, only in *Trichobatrachus robustus*, an anamniote without appreciable keratinization, are hair-like projections of the skin used as supplemental gas exchange surfaces (Noble 1925).

REPRODUCTION

Bousfield and LeBlond (1995) suggest live birth for *Cadborosaurus*, citing the general features of viviparous reptiles to be precociality and the small size of offspring. The former is certainly true, but most live-bearing reptiles (except for snakes) give birth to few very large young. *Trachydosaurus rugosus* is a case in point. In this large Australian skink, single or twin births are the rule, and body mass of the newborn may be 20 percent of the post-parturition weight of the female (Green 1995). Live-bearing geckos also produce twins of large size (Cree 1994).

Although they do not fully endorse this interpretation, Bousfield and LeBlond (1995) consider that *Cadborosaurus* may be anadromous. This would require that adults of 65–100 feet (20–30 m) in length migrate up

rivers perhaps as much as 100 miles (160 km) in order to give birth. Such an animal may go unnoticed in the vastness of the ocean, but probably not in a narrow river channel. It is assumed that, unlike anadromous fishes such as salmon, which spawn and die in fresh water, *Cadborosaurus* adults return to the sea (or else there would certainly be an occasional observation of the large rotting carcasses in inland waters). Although the saltwater crocodile, *Crocodylus porosus*, and other species of living "reptiles" are capable of surviving in either salt water or fresh water, none have anadromous life cycles, and no known living form that is essentially obligately aquatic makes the shift.

Some of the features attributed to the newborn *Cadborosaurus* are not evident in the illustration by Haglund provided by Bousfield and LeBlond (1995). The yellow fuzz reported on the venter is certainly not compatible with any "reptilian" structure, but might represent a mass of small yellow eggs carried on the abdomen by a crustacean. The tail illustrated looks very fishlike, but might also represent the telson of a decapod. In any event, the rather drastic differences between juvenile and adult or subadult in *Cadborosaurus* suggest that metamorphosis of some type must occur, yet no amniotes undergo metamorphosis. In *Pachypleurosaurus*, the proportions of bony elements remain relatively constant across the range of body sizes known (Carroll and Gaskill 1985), in contrast to the situation in *Cadborosaurus*, in which there are supposedly marked allometric changes in ontogeny.

LIFESTYLE

The reconstruction of the lifestyle of *Cadborosaurus* is entirely conjectural, and many of the points made by Bousfield and LeBlond (1995) are incompatible with what is known of plesiosaurs. The use of nasal passages as resonating chambers is suggested. This has been demonstrated in some archosaurs, including hadrosaurian dinosaurs (Hopson 1975) and modern birds (Alexander, Houston, and Campbell 1994). In these organisms, however, this was only determined through careful comparative anatomical techniques unavailable for *Cadborosaurus* because of the lack of material. Further, even in the living hornbills, the role has not been established unambiguously. Echolocation has also been proposed for *Cadborosaurus*. However, Taylor (1992) and Taylor and Cruickshank (1993) indicated that there was no acoustic isolation of the otic capsules in pliosaurs or plesiosaurs, suggesting that they had no form of echolocation.

The fish-trap jaws (not seen in the Naden Harbour specimen nor illustrated in the eyewitness drawings presented by Bousfield and LeBlond 1995) said to be a feature of *Cadborosaurus* are generally associated with obligate fish feeding. Nonetheless, it is reported to feed on squid at depth and on seabirds at the surface. The assumption that feeding is snakelike is totally untenable.

A chief feature of snake feeding is streptostyly (the condition of having a highly mobile quadrate), which is absent in plesiosaurs.

The reported distribution of *Cadborosaurus* in the water column is pure speculation. The habitat of the animal is presented rather authoritatively as depths close to 3,300 feet (1,000 m). This is based on the fact that the 1937 specimen was recovered from a sperm whale stomach, and that this is near the deepest record or estimate of sperm whale diving.

Individually, these reconstructions of the lifestyle of *Cadborosaurus* from virtually no evidence might be excusable, but, together, along with other aspects not discussed here, they form an elaborate and totally unfounded picture of an animal that may not even exist. The describers of rare and poorly known animals are certainly entitled to speculate on the habits of their new finds. But such speculation is generally grounded in the morphology of specimens actually in hand, and on inference from closely related forms about which more is known. Given that neither are available for *Cadborosaurus*, we believe that it would have been prudent for Bousfield and LeBlond (1995) to eschew discussions of pheromones, echolocation, and other biological traits not preserved and visible in the photos of the holotype of *C. willsi*.

WHAT IS *CADBOROSAURUS*?

We have attempted to demonstrate that Bousfield and LeBlond (1995) have not provided convincing evidence for a living plesiosaur in the waters of British Columbia. If we are correct, what, then, is *Cadborosaurus*? Or, more appropriately, what are the sources for the evidence presented for the existence of a cryptozoan in the Straits of Georgia? In invoking Ockham's razor, we should look for the simplest explanation that fits the available data. We believe that known organisms can most parsimoniously account for the testimonial evidence regarding "adults," the "sub-adult" holotype in the Naden Harbour photograph, and the "newborn" specimen seen by W. A. Haglund. Each presumptive life stage is clearly different from the others in some aspects, and we regard it most likely that a different kind of animal has served as the basis for each stage.

The most significant evidence for *Cadborosaurus* are the physical remains photographed in 1937. Bousfield and LeBlond (1995) claim, without justification, that the Naden Harbour specimen was intact, ruling out the possibility that it had been partially digested or decomposed. However, there is a long history of very similar-looking Pacific "sea monsters" that have turned out to be decomposing sharks. A somewhat similar animal, although less fully decomposed (if we are correct), was found by a Japanese trawler off the coast of New Zealand in 1977. It too was thought to be a plesiosaur by some (Koster 1977), but was in all probability the carcass of a shark (Anonymous 1977).

More pertinently, there is also a long history of such animals from the waters of coastal British Columbia. Bousfield and LeBlond (1995) appear to have ignored very significant passages and figures from Heuvelmans (1968), who provided information regarding similar "sea serpents," such as that reported from Henry Island in 1934. The Henry Island monster was "the rotting carcass of an animal about 30 feet in length." It "was reported to have reddish flesh, skin covered with hair mixed with quills, a horse's head and a spinal column upon which one could see what seemed to be where four fins or flippers were attached." It was thought by many at the time that this animal was indeed the "sea serpent" known locally as *Cadborosaurus*, or Caddy.

The skull and some parts of the vertebral column were examined by the director of the biological station at Nanaimo, and declared to be the remains of a basking shark. However, the keepers of the Provincial Museum, in Victoria, reportedly stated that the remains were those of a Steller's sea cow, a thought-extinct sirenian. Comparisons of the carcass with the skeletons of a sea cow and a basking shark revealed that no particular resemblances between the first two could be noted, but that similarity between the carcass and the basking shark was very marked: "The shape of the skull, the position of the eye-socket, the shape of the vertebrae and even their approximate number, the dorsal ligament joining them together, all agreed exactly. The hairy horse-headed monster of Henry Island was therefore a decomposed large selachian—most likely a basking shark" (Heuvelmans 1968). Plate Fig. 16 in Heuvelmans (1968) shows the remains of the Henry Island monster to be very similar to the *Cadborosaurus* photo in Bousfield and LeBlond (1995), only larger. Another animal, washed up at Effingham in 1946 and also supposed by some to be Caddy (see Heuvelmans 1968), also turned out to be the remains of a basking shark.

In the same year that the Henry Island monster appeared, a similar phenomenon occurred in France, and, again, wildly speculative interpretations were made. The carcass in this case, again a basking shark, was associated with earlier sightings of horse- or camel-headed, fast-moving, serpentine sea creatures. This specimen, known as the Querqueville monster, was less decomposed than the Henry Island animal, which was more like the holotype of *Cadborosaurus* in overall appearance. It should be noted that, in both the Canadian and French examples, seafaring men, who would have recognized the intact animal, were not able to identify the partial remains. On the basis of the Querqueville monster and other cases, Heuvelmans (1968) concluded: "It seems to be quite normal for basking sharks to take on a plesiosaur's shape when they decompose. This is because of the peculiar structure of their gill-slits which are extremely long and go almost right round the neck . . . as soon as its tissues decompose and become soft the whole gill apparatus falls away taking the jaws with it, so that nothing remains in front of the

pectoral fins but the tiny skull and the spinal column clad in its muscles and thus looking like a thin neck. At the other end of the body, the lower fluke of the tail soon goes with nothing to support it, since the spine extends only into the upper one. The body then seems to have a long thin tail." Heuvelmans (1968) also indicated that decomposition of the skin results in a body that gives the appearance of bearing hair, and that remnants of the dorsal fin may result in a mane-like appearance in the region of the "neck."

Basking sharks are common in British Columbian waters (Compagno 1984), and this seems to be the most likely candidate for the Naden Harbour carcass. Whereas sperm whales may not be known to feed on basking sharks, neither have they been reported to feed on plesiosaurs. This (basking shark) interpretation could be consistent with Francis Kermodé's identification of part of the Naden Harbour carcass as "baleen" (see Bousfield and LeBlond 1995) if one takes into account the fact that either the gill rakers of basking sharks or ceratotrichia of the fins of any elasmobranch might be mistaken for this material by someone unfamiliar with these structures.

What of the observations of "adults" at sea? Again, we think that known animals provide the most parsimonious explanation. Our criticism of testimonial evidence is not meant to imply that witnesses have lied, but rather that they may have misinterpreted what they saw. This is easy to do. Size is essentially impossible to estimate at even moderate distances against the featureless background of the water surface, especially since, as the authors admit, these animals are seen only for short periods at the surface. Further, witnesses are usually surprised by the sudden appearance of the animal, and, once they become oriented, have little time to interpret what they see. The sounds of snorting or blowing are consistent with breaching marine mammals. The large eyes reported seem improbable for very large vertebrates, such as whales, as these tend to have relatively small eyes.

Although there are clearly some inconsistencies, we regard pinnipeds, especially the northern elephant seal, *Mirounga angustirostris*, as the most likely candidates for the source of most of the observations reported at sea. These large mammals, measuring up to 16 feet (5 m) (King 1983), are seldom seen at the surface during their annual feeding migrations, which take them past the shallow coastal waters of British Columbia (Banfield 1974). Indeed, they have recently been reported to surface for only 1.6 minutes, with dives lasting as long as 48 minutes (Le Boeuf et al. 1988, Campagna et al. 1975). Significantly, northern elephant seals were only relatively recently confirmed to frequent the coastal waters of British Columbia (Banfield 1974), this being demonstrated in 1944, seven years after the finding of the Naden Harbour specimen. The most problematic aspect of the eyewitness accounts is the reference to the long, slender neck. This leaves the tantalizing possibility that an unknown marine animal having this feature lives in these waters, but such sightings certainly do not justify making the connection to the

Naden Harbour specimen. Thus, we contend that such sightings are unconnected with any physical evidence, and must remain an intriguing side issue to the current case.

The "newborn" *Cadborosaurus* is the most difficult to associate with known forms, and it also combines the most unlikely suite of features. Nonetheless, we feel that the description is perhaps compatible with some decapod crustacean, with the "yellow fuzz" on the abdomen representing an egg mass, and the "overlapping flipper-like fins" of the tail the telson of such an invertebrate. Certainly, whatever the "newborn" was, or is, there seems little, other than geographic proximity, to link it to either the Naden Harbour specimen or the testimonial evidence regarding adult *Cadborosaurus* at sea.

CONCLUSIONS

Bousfield and LeBlond (1995) claim that the Naden Harbour photo of the holotype is as good as physical fossil evidence for establishing the existence and identity of *Cadborosaurus*. However, in the absence of realistic models, fossils or photos may be grossly misinterpreted. A case in point is *Hallucigenia* from the Burgess shale, which had been variously interpreted as part of some larger organism, or as a representative of a new and distinct phylum. The discovery of allied fossils, and the recognition that earlier reconstructions had placed the specimen upside down, have revealed that the animal is an onychophoran, or at least a close relative thereof (Ramsköld and Hou 1991). Likewise, conodonts, which remained known only from their enigmatic mouthparts, are now recognized to be vertebrates (Gabbott, Aldridge, and Theron 1995).

It is our contention that Bousfield and LeBlond (1995) selected an inappropriate model for their interpretation of the evidence, and forced improbable conclusions from this. We regard as a fundamental flaw of their interpretation the insistence that three apparently disparate sets of putative information—testimonial evidence, evidence from the specimen photographed, and evidence regarding the tiny "newborn"—are related to one another, and that all, despite their almost total incongruity, refer to one animal.

It is our view that most of the testimonial evidence fits rather well with a pinniped, perhaps the elephant seals that migrate through the area on their way north to their feeding grounds in Alaskan waters. The sighting reports of long-necked animals remain intriguing, but appear to be unrelated to the physical evidence in the photographs, as the morphology is not congruent. The evidence from the 1937 Naden Harbour photos clearly supports a decaying shark, perhaps a basking shark, as the culprit. And the very odd "newborn" is most compatible with a crustacean, or perhaps a teleost fish of some kind.

Bousfield and LeBlond (1995) cite evidence for the discovery of large,

previously unknown or ethnoknown animals as a basis for the plausibility of their own findings. While we do not deny the possible existence of such cryptozoans, we advocate a rigorous approach to their verification. Analysis of such phenomena require that Ockham's razor be invoked, and that explanations that are consistent with known phenomena be accepted as simpler working hypotheses before more tenuous interpretations are embraced. The onus is on Bousfield and LeBlond (1995) to demonstrate why such less controversial interpretations cannot be accepted before building complex arguments based on purely speculative underpinnings.

Albert (1987) argued for specialization in cryptozoology, and we concur. To spread oneself too thin is to make errors, and that is what has occurred in the formal naming of *Cadborosaurus willsi*. In describing a species in a group with which neither author is professionally familiar, Bousfield and LeBlond (1995) have made unwarranted assumptions, and have bypassed virtually all of the appropriate literature that would have channelled them towards some more reasonable interpretation of *Cadborosaurus*.

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(Readers wishing to consult the original paper describing *Cadborosaurus willsi*, including the accompanying illustrations, should contact: Edward L. Bousfield, Managing Editor, *Amphipacifica*, 611-548 Dallas Rd., Victoria, British Columbia, Canada V8V 1B3—Editor.)

ON A POSSIBLE UNKNOWN SPECIES OF GIANT DEVIL RAY, *MANTA* SP.

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ABSTRACT: This paper discusses the possibility of an unknown species of giant devil ray, *Manta* sp., distinguished by a dorsal color pattern of symmetrical V-shaped areas of brilliant white bands, a non-falcate rhomboid body configuration, and a relatively short tail. Other undescribed giant mantas and giant fresh-water rays are also discussed.

INTRODUCTION

On April 27, 1923, the American naturalist and explorer William Beebe, aboard his expedition vessel *Noma*, which was approaching Tower Island (also called Genovesa) in the Galapagos Archipelago, observed a large devil ray or manta, of which he immediately made a careful sketch. He later gave the following description:

A few minutes after six, standing forward close to the starboard rail, several of us saw a very large ray or devil-fish struck a glancing blow by the yacht, and go rushing off along the surface. Realizing what a splendid chance this was for definite observation, I concentrated on the colour and pattern, and when it vanished, I sketched it at once, the details being verified by my companions.

From tip to tip of wings it was at least ten feet, of somewhat the usual manta or devil-fish shape, except that the wings were not noticeably concave behind, and the lateral angles were not acute. The cephalic horn-like structures were conspicuous and more straight than incurved. In general the back was dark brown, faintly mottled, while the most conspicuous character was a pair of broad, pure white bands extending halfway down the back from each side of the head. The wing tips also shaded abruptly into pure white (Beebe 1924: 312).

PHOTOGRAPHIC EVIDENCE

There have been, so far, no attempts at determining the taxonomic status of this fish, which Beebe himself called an “unknown species” (Fig. 1). In the course of a recent study of manta photographs, another specimen from New Caledonia was found to show the same symmetrical white dorsal bands, particularly when the original color photograph (in Fourmanoir and Laboute 1976) was subjected to the process of black-and-white photocopying (Fig. 2). Another photograph (by P. Kopp, in Wheeler 1977) shows an obviously distinctly bordered white band on the right shoulder (the left shoulder is not visible) of a supposed giant devil ray (*Manta birostris*); this may be yet another specimen belonging to the unknown taxon.

An affirmation came when, on December 28, 1989, a documentary film



FIG. 1.—William Beebe's sketch of an unknown devil ray or manta. Probably male. From Beebe (1924).

was shown on German television (3rd Program, NDR [North German Television], N3) titled *Sharks: Hunters of the Seas*, narrated by Heinz Sielmann, and filmed and produced by Sigurd Tesche, of S. Tesche Productions. There is a sequence of more than 30 seconds showing a large manta at close quarters in the waters off Cabo San Lucas, at the southern tip of Mexico's Baja California. The fish showed magnificent, brilliantly-white symmetrical V-shaped bands on its dorsal surface.

The producer, Sigurd Tesche, promised me still photos of the fish, and he also agreed to go through the material not used in the film (personal communications, February, 1990). Several attempts at obtaining this material have proved so far unsuccessful. Consequently, until this material is made available, photographs taken off a television screen will have to suffice (Figs. 3–5).

An unknown species of devil ray may be involved. However, neither Beebe's sketch nor the photographs justify a formal description of a new species at this time; the sketch would seem to be generalized, and the photographs are not helpful in assessing morphometric details due to the perspectives and angles of the fish in motion. Therefore, in order to create an

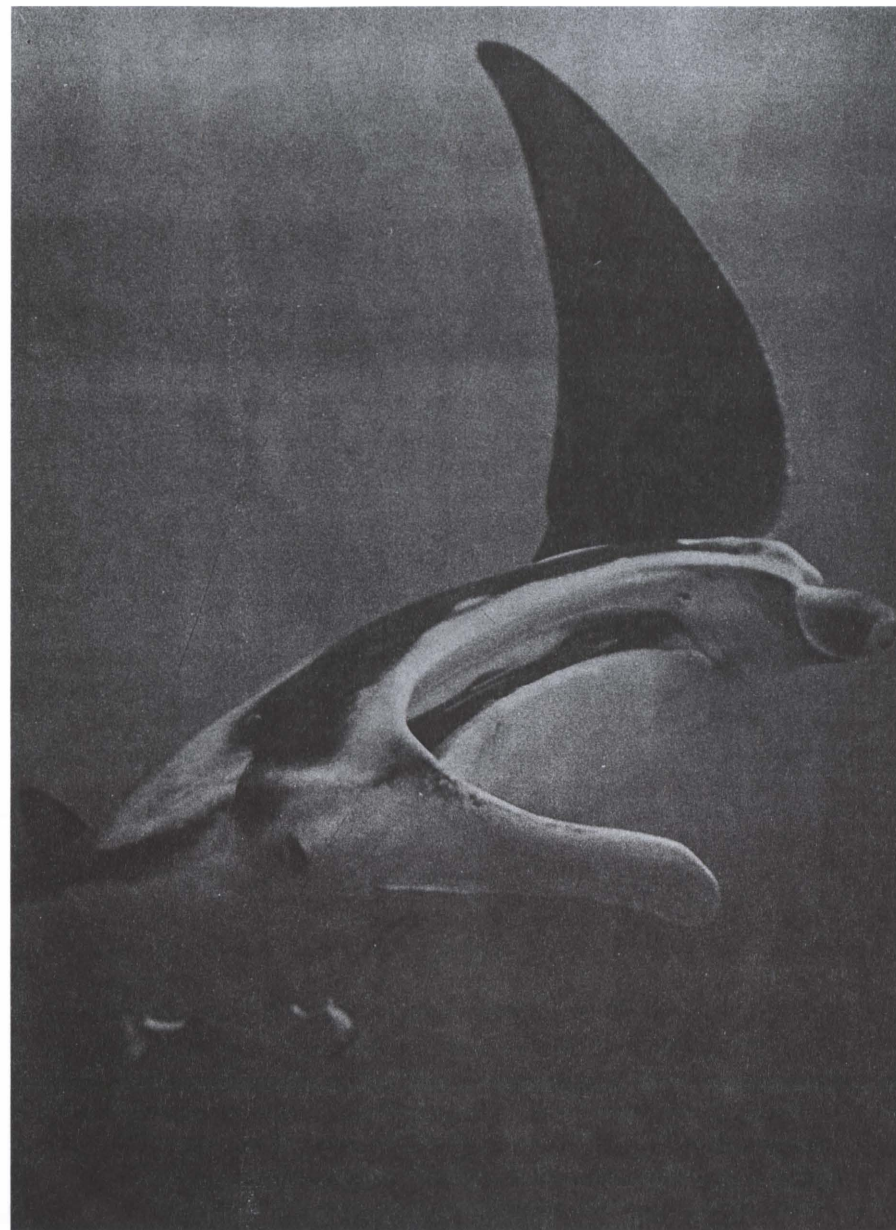


FIG. 2.—Unknown devil ray or manta from Ouvea, New Caledonia. Probably female. From Fourmanoir and Laboute (1976).

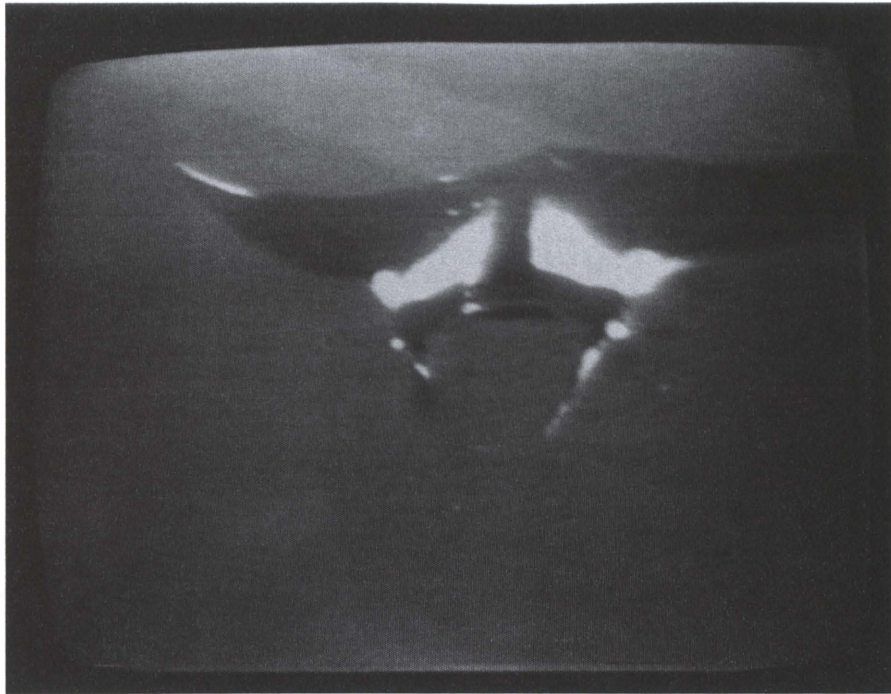


FIG. 3.—Unknown devil ray or manta filmed off the coast of Baja California by Sigurd Tesche, broadcast in December, 1989. This illustration is a “quotation” according to German Copyright Law (§ 51 i.V.m. §§ 94 IV, 95 UrhG).

awareness of this potential new species, and initiate the search for further evidence, it seems more appropriate to present the case on the basis of the available material.

PRESENT TAXONOMIC SITUATION

A giant ray as depicted by Beebe and appearing in the photographs should be classified as either an eagle ray (Family *Myliobatidae*) or a devil ray or manta (Family *Mobulidae*). The taxonomic structure of the latter family is uncertain and still debated. There are thought to be either two or four genera comprising a still unknown number of species (Klausewitz 1970), or two genera and 10 species (Nelson 1976), or “several species” (Smith and Heemstra 1988), or they are “all poorly known” (Eschmeyer, Herald, and Hammann 1983). According to the minimum consensus discernible, the *Mobulidae* contain two genera with nine species: genus *Mobula* Rafinesque, 1810, with seven valid species recognized, and genus *Manta* Bancroft, 1829, with two valid species. Whereas *Mobula* has the mouth on the lower surface of the head (ventrally), and a tail twice as long as the body, *Manta* has a terminal



FIG. 4.—Unknown devil ray or manta filmed off the coast of Baja California by Sigurd Tesche, broadcast in December, 1989. This illustration is a “quotation” according to German Copyright Law (§ 51 i.V.m. §§ 94 IV, 95 UrhG).

mouth extending anteriorly across the front of the head, and a tail conspicuously shorter.

The fish depicted by Beebe and appearing in the photos has a broad terminal mouth and, on Beebe's sketch, a relatively short tail. The line between the lobes on each side of the head is not concave as in *mobulas*. It is therefore more appropriate to assign the fish to the genus *Manta*, particularly as Beebe himself described a “manta or devil-fish shape” (Beebe 1924: 312). Beebe's manta, as it may tentatively be called, differs considerably from the well-known species *Manta birostris*, the giant devil ray. These differences justify a discussion, and the question of whether or not a new species or subspecies is involved.

DISCUSSION

The material presented here concerns a devil ray or manta of large size, Beebe's sketch depicting an individual of at least 10 feet (3 m) across the



FIG. 5.—Unknown devil ray or manta filmed off the coast of Baja California by Sigurd Tesche, broadcast in December, 1989. This illustration is a “quotation” according to German Copyright Law (§ 51 i.V.m. §§ 94 IV, 95 UrhG).

pectoral fins (“wings”) from tip to tip. The fish differs from the recognized species of the genus *Manta* in having a dorsal color pattern of symmetrical V-shaped areas of brilliant white bands, a seemingly rather “square” diamond-shaped (rhomboid) non-falcate silhouette or body configuration, and a tail considerably shorter than in the known manta species.

Color Pattern: The most obvious character of Beebe’s manta is its unique dorsal color pattern. The dorsal color is dark brown, with barely discernible dots, “faintly mottled” (Beebe 1924). On the “shoulders” there are two distinctly-bordered, separated but symmetrical areas of brilliant white blotches or bands. Although the dark pigmentation of the well-known giant devil ray (*Manta birostris*) is easily rubbed off—so that the superficial areas of coloration may be natural in some individuals and accidental in others (Migdalski and Fichter 1976)—this cannot have happened to Beebe’s individual, or to those of New Caledonia and Baja California. The white areas are distinct on Beebe’s sketch, as well as on the photographs, the otherwise dull dorsal color shading “abruptly into white” (Beebe 1924). They are clearly bordered, and absolutely symmetrical. Rubbed-off pigmentation cannot produce bril-

liant white areas (no such case is on record), but rather *some* irregularly-spaced *lighter* patches.

The photographs show a distinctly-bordered, symmetrical design (slightly different from Beebe’s sketch, as the bands seem to curve toward the vertebral mid-line). The individual from New Caledonia (Fig. 2) has white interior and exterior sides of its cephalic fins (“horns”); in Beebe’s specimen (Fig. 1), only the interior areas are white. This may be due to sexual dimorphism. The broad hind edge of Beebe’s illustration may indicate the presence of genital claspers, so that the individual would have been a male; thus, the individual from Ouvea would have been a female. The white edges of the rounded wing tips on Beebe’s sketch, which the Caledonian and Baja Californian individuals do not show, may have been an illusion due to the manta’s mode of swimming by curling up the wing tips dorsally and inward, so that the white ventral surface shows, and the falcate tips appear to be absent.

Herre (1936), after having observed *Manta birostris*, stated: “They have a habit of swimming a few feet below the surface with upturned triangular tip of each gigantic wing-like pectoral fin projecting above the water.” Also, Migdalski and Fichter (1976) mentioned “the visible parts of the curled-up tips of the huge wings.” Therefore, falcate wing tips should be restored to Beebe’s illustration, his white “wing tips” providing evidence that the ventral surface is white.

There are numerous explanations for unusual color patterns in fishes that do not necessarily relate to taxonomic differences. Specifically, unusual markings can be color variations that may not even have subspecific status, but may be of a so-called “forma” (e.g., both black and spotted individuals are “forms” of the single species of leopard as they occur in one litter). Color variations of a lesser nature do occur in this group of fishes, but extreme variants as shown by the material presented here are unknown. Nevertheless, color pattern alone cannot constitute decisive evidence for a distinct taxonomic status.

Body Configuration: The body configuration as given by Beebe shows a more regularly diamond-shaped silhouette than in *Manta birostris*. Anterior edges of the pectoral fins are not strictly convex, and posterior edges not strictly concave, as in *M. birostris*. Even with wing tips restored to Beebe’s drawing, and the more “elegant” photographs taken for guidance, the wingspan of Beebe’s manta is approximately one and a half times the body length, whereas in *M. birostris* it is twice the body length. However, no morphometric details can be gathered from the photographs because of perspective and angle of the fish in motion. There is no way of processing the photographs in order to obtain morphometric data. Therefore, the body configuration cannot be restored with the necessary degree of authenticity to warrant a conclusion.

Tail Length: The tail as drawn by Beebe is shorter than the body length (ratio 0.6:1); it is not serrated. A large dorsal fin is present at the base of the tail. In Tesche's film, tail length seemed to be half the body length. In *M. birostris*, tail length is equal to body length (Makuda *et al.* 1984).

Further Possible Evidence: Fowler (1927) described, as belonging to *M. birostris*, the fragments of a small specimen of devil ray harpooned off the shore reef at Fanning Island in 1924. They were supplied to him with sketches, notes, and details by Stanley C. Ball, a medical doctor. Fowler described the fish as follows: "Disk nearly twice as wide as long [not verified by Fowler's own illustration]; tail broken terminally so now less than disk length. Pectorals rather narrowly triangular, front edge slightly convex, hind edge concave . . . Back blue-black, with two v-shaped bands, angles forming at vertebral line and pointed backward . . . The distance from tip of snout to caudal base given as 112 cm on Ball's sketch, also width of disk same [contrary to his own text above] . . . The specimen noted by Ball differs from any I can find in its coloration, possibly a condition of youth" (Fowler 1927).

The peculiar disk dimensions (printer's error rectified), as well as the unique color pattern (Fig. 6), which is not that of a young *M. birostris* (they are unbanded), hint at another specimen wrongly assigned to *M. birostris*. The double v-shaped band and its "ashy" color may be a condition of youth in Beebe's manta. Or, Ball's specimen may constitute yet another unknown manta species. It was, in fact, subsequently described as a new species, and named in honor of Fowler: *Manta fowleri* Whitley, 1934. Since then, Fanning Atoll has been famous for the numerous research dives for mantas that have been made there. But no further specimen has been found. Though Fowler is known to have made errors in his drawings of fish color patterns (a printing[?] error has been pointed out above), it is not at all likely that, in this particular case, with a naturalist and medical doctor for witness, Fowler confused two dorsal bands with one. It is more probable that he exercised extreme care in drawing the specimen.

FUTURE RESEARCH

Further fieldwork is necessary to obtain additional evidence—preferably a specimen—which might then lead to a formal taxon description and naming, if indeed a new species or subspecies is involved.

In the past, dozens of *Manta* species have been described under various names. Today, many of them are considered synonyms of a single species (Smith and Heemstra 1988); others remain apocryphal. As they were described from single specimens, almost always badly damaged and fragmented by the process of being washed up by the sea, these are in serious need of re-identification and revision. An unknown number of species awaits discovery. On mantas, Whitley (1936: 11) stated: "There are other species,



FIG. 6.—Mysterious specimen from Fanning Island, as illustrated by Fowler (1927).

some yet unnamed scientifically, in the Red Sea, off the coasts of Africa, India, the East Indies, South America, and the South Sea Islands. Some of these exceed twenty-four feet in width."

Not even the earliest known engraving of an East Indies devil ray (Nieuhof 1682: Fig. 7) has been identified. Seven pairs of zebu oxen had to be employed to drag this monster ashore. The flesh was said to taste like venison. Although the lobe-like appendages are evidence that the fish belongs with the mantas of the family Mobulidae, there is no known member with seven spines on the tail (Fig. 7). This giant species has never been reported again.

There are, however, continuous reports on giant fresh-water rays from the Mekong River (Thailand and Laos). A giant stingray is reported to be caught in pools and puddles (when the river system dries up annually), and sold in local markets. In 1983, Bangkok newspapers featured the photograph of a specimen from Nakhon Phanom, Thailand, which had a disk diameter of 7 feet, 10 inches (2.4 m), with the tail another 7 feet, 10 inches (2.4 m), and a weight of 532 lb. (242 kg). The fish is said to reach more than 10 feet (3 m) in diameter. This unknown species probably constitutes a larger congener of the small, recently discovered Mekong stingray *Dasyatis laoensis* Roberts and Karnasuta, 1987.

Of similar dimensions, said to reach 6 feet, 6 inches (2 m) in diameter, is another unknown stingray from the Chao Phraya River, Thailand. A German

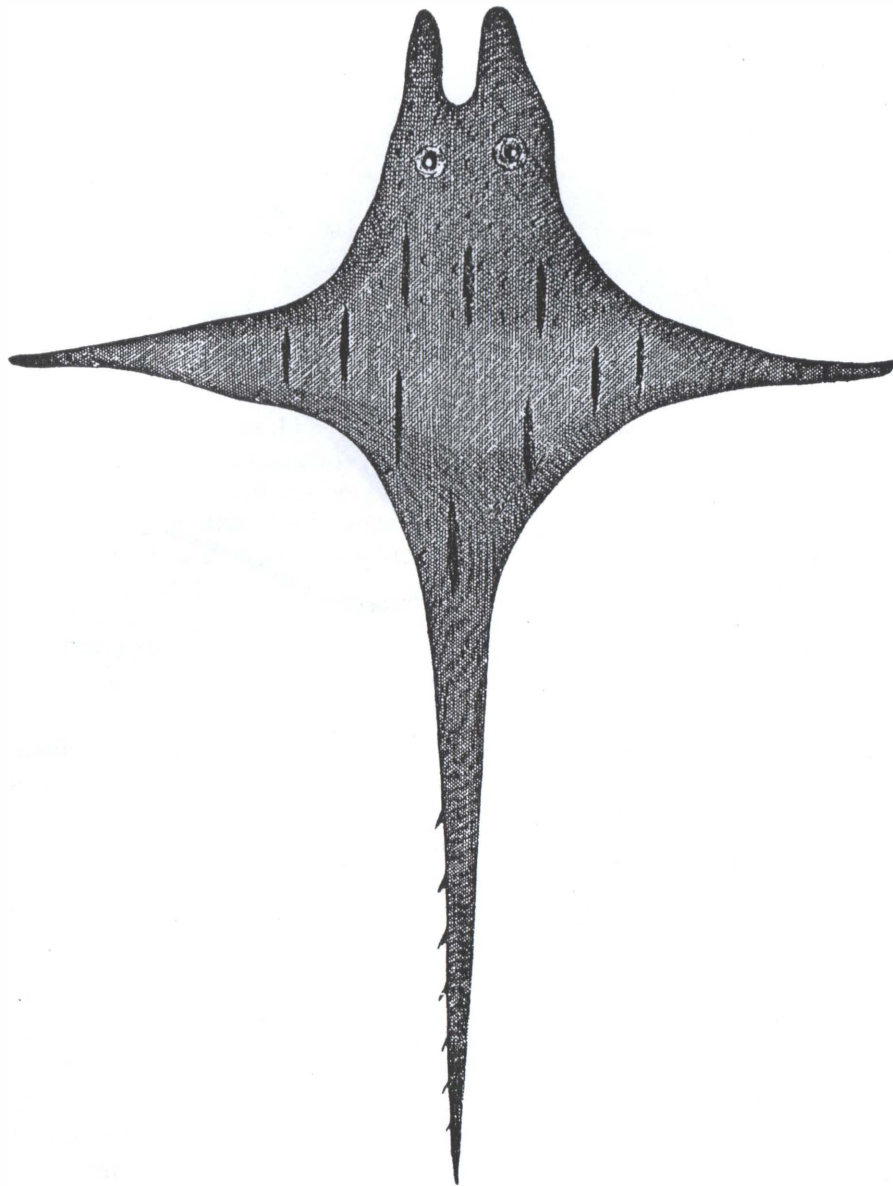


FIG. 7.—Mysterious specimen from the East Indies, as illustrated by Nieuhof (1682).

ichthyologist was recently shown photographs of such giants, as well as a live young specimen of 32 inches (80 cm) in diameter by a Bangkok pet fish exporter (Kottelat 1989). The young ray, as photographed, constitutes a *Himantura* species, but certainly not the well-known *Himantura fluviatilis*.

In cryptozoology, collecting reports, sketches, and photographs is just a first step. The ultimate goal of obtaining a specimen, and of describing and naming a new species, would not seem to be too far-fetched in these instances. The flying devils of the deep blue sea, as well as the deadly giants of South East Asia's muddy rivers, offer a truly "gigantic" field for future research.

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TACHYGIA, THE GIANT TONGAN SKINK: EXTINCT OR EXTANT?

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(Dedicated to the memory of John R. H. Gibbons, who searched the central Pacific
for known and unknown lizards.)

ABSTRACT: The giant tongan skink, *Tachygia microlepis*, is known from only two specimens held in France's National Museum of Natural History, in Paris, and collected in the early 19th century. Possible recent sightings of this lizard are reported and explained. They almost certainly correspond to another species, *Eomoia trossula*. Available evidence indicates that *Tachygia* is now extinct on Tongatapu, but we cannot conclude this conclusively.

INTRODUCTION

An assortment of giant reptiles once lived on the widely scattered islands of Oceania. In the east, giant tortoises occurred on all of the Galapagos Islands. The surviving tortoises share the Galapagos with several large species of iguanas, *Conolophus* inland and *Amblyrhynchus* along the coast. On the western Pacific islands, no lizards match the Galapagos iguanas in size, but giant geckos, *Rhacodactylus* of New Caledonia and *Hoplodactylus* from New Zealand, surpass the maximum size of any other geckos.

Less well known are the giant skinks of the Pacific, *Phoboscincus* and *Tachygia* (these two genera are considered synonyms of *Eugongylus* by some systematists, e.g., Böhme 1976). These skinks are giants in a relative sense, being the largest ones in the Pacific. *Phoboscincus*, of New Caledonia, has two species, both reaching total lengths of more than 500 mm (*P. bocourti*, 280 mm snout–vent length [SVL]) and 400 mm (*P. garnieri*, 200 mm SVL). The species *P. bocourti* is known from only a single specimen, with “New Caledonia” as its location of origin, plus possible sightings in the 1980's (see Bauer and Vindum 1990: 35). The species *P. garnieri* is somewhat better known, represented by eight specimens in European museums. One of these (Paris MNHN 1989.13) was recently found on a mine track at Mont Mou, in southwestern New Caledonia; this specimen is 210 mm long (165

mm SVL). Another recent specimen is in the Australian Museum, and was collected in 1992 by Ross Sadlier and Aaron Bauer in northeast New Caledonia. Both recently collected specimens proves the survival of this giant skink.

In the central Pacific, Fiji possesses an iguana, small (220 mm SVL) when compared to its American relatives, but the giant of the local lizard fauna. Tonga has another giant, the skink *Tachygia microlepis*. *Tachygia* is known from only two specimens, collected by Jean René Constant Quoy and Joseph Paul Gaimard, two famous “médecins-naturalistes” of the 19th century French Navy. Between 1817 and 1820 they traveled with Louis Claude Desaulles de Freycinet on the *L'Uranie* circumnavigation and scientific exploration of the world. Later, from 1826 to 1829, on their second circumnavigatory exploration with Jules Sébastien César Dumont d'Urville, on the frigate *L'Astrolabe* (formerly named *La Coquille*), they collected both specimens of *Tachygia* and brought them back to Paris. There is little likelihood of an error concerning the origin of these specimens, although these giant skinks are not mentioned in the travelogue of Dumont d'Urville (1830–1833); he reports only a sea snake collected at Tongatapu (Vol. IV: 331–4).

The two *Tachygia* could not have come from New Zealand because Quoy and Gaimard state (in Dumont d'Urville 1830–1833: Vol. II: 592): “Sauf quelques petites espèces de lézards, nous ne connaissons point d'autres reptile (sic)” about their experiences in New Zealand. The French expedition spent one month in Tonga (April 20–May 21, 1827), and although they made no comment on any lizards, they had adequate time to explore the islands.

Since the initial discovery of the giant Tongan skink, no additional specimens are known to have been collected. During his 1985 teaching tour in Tonga, John R. H. Gibbons searched for *Tachygia*, and even offered a reward (Fig. 1). He did not have an opportunity to report his findings before his and his family's tragic deaths. Since he discussed his search for the giant skink with both authors, we wish to summarize his findings in this paper.

POSSIBLE RECENT SIGHTINGS OF TACHYGIA

Gibbons visited Tongatapu and 'Eua in January and October, 1985. While his visits were brief on both islands, he collected over 90 lizards. As an aside, his inquiries about large lizards revealed that *Brachylophus fasciatus* still survives on Tongatapu (although he observed none), but that, within memory of his witnesses, *Brachylophus* has not been seen on 'Eua.

Much of Tongatapu has been drastically altered by a millenium of human occupation and agriculture. A single stand of native forest remains in south-central Tongatapu, between Tupou College and the airport. It has also been modified, but owing to its use as a religious site in pre-European times, the Tongans continue to preserve it as a forest. As the only remaining area of

If You See A Grey Ghost, Don't Run, Take Its Photo

A grey ghost last seen here nearly 150 years ago may be stalking about Tongatapu. A University of the South Pacific specialist is determined to track it down.

The grey ghost is actually a lizard with the jawbreaking scientific name of *Tachygia microlepis*. Only two specimens have ever been found. Both were collected on Tongatapu by a French expedition in the 1800s and are presently preserved in a Paris museum.

Meanwhile, Dr John R.H. Gibbons, a researcher and lecturer in the Biology Department, School of Pure and Applied Sciences at the USP Laucala campus in Suva, Fiji, has been in the Kingdom

black markings.

Last October Dr Gibbons and students of Tupou College conducted a search for the grey ghost between the college and Fua'amotu within the last stand of native Tongatapu forest. Although they found specimens of 10 of the 11 lizard species known to inhabit the island, they did not find the grey ghost.

Rare as it apparently is, the ghost figures in Tongan legends. According to one, sighting it is an omen of a major family event, such as a wedding or funeral. The lizard also figures in a legend associated with the founding of the noble family of the Hon. Fakafanua, Governor of Ha'apai.

Dr Gibbons believes that the grey ghost and other lizards were once abundant on Tongatapu. But, as more and more bushland has been taken over for plantations and with the introduction of cats and rats to the island, only "a tiny remnant" remains.

Dr Gibbons, who plans to return to Fiji on Saturday, has requested that anyone who can photograph the grey ghost or collect a specimen contact Mr Jim Foster of Tonga High School. Mr Foster can be reached after school hours at Govt Qtr 76, Taufa'ahau Road. A \$100 reward is being offered for photograph or specimen proved to be the grey ghost.

NOTICE

\$100 REWARD
for specimen of reptile known as
THE GREY GHOST, MOKO LAHI, or TACHYGIA MICROLEPIS

The lizard is thought to be 50-60cm from head to tail. Only two have ever been found in the world. These were collected on Tongatapu in the 1800s by a French expedition. Their remains are now housed in a Paris museum.

The body colour is thought to be dull grey and the shape like a giant *pili pili*. The reptile probably lives underground, only coming out at night or after a heavy rain. Little else is known about it.

We hope to find out if the lizard still lives in Tonga. If so, we would like to collect as many specimens as possible as part of a USP survey on reptiles (*moko, ngata, fonu, fokai*, etc.) of Fiji, Samoa, and Tonga.

Telephone Mr Jim Foster of Tonga High School or call at Govt Qtr 76, Taufa'ahau Road (opposite Royal Tombs) after 4p.m. with any specimens or evidence you may find.

If you have collected a specimen likely to be identified as the grey ghost, it will be sent to the USP in Suva and your reward will be paid by return post.

FIG. 1.—Extract from the *Tongan Chronicle* of February 21, 1986, showing John Gibbons' *Tachygia* reward notice.

native vegetation, Gibbons chose it for an intensive search. On October 22, he and several Tupou College students "combed" the forest for lizards, and found many of the known Tongatapu species: *Gehyra oceanica*, *Gehyra mutilata*, *Lepidodactylus lugubris*, *Hemidactylus garnotii*, *Nactus pelagicus*, *Emoia cyanura sensu lato*, and *Lipinia noctua*. Only *Cryptoblepharus eximius* (a coastal species), *Hemiphyllodactylus typus* (often a human commensal) and *Emoia trossula* were not encountered. The latter species is, however, represented by a preserved specimen in the Tupou College collection.

Even though the physical search for *Tachygia* failed, Gibbons's inquiries indicated the existence of very large skinks on Tongatapu. For various reasons, most notably lack of detail and suspected unreliability of the witnesses, Gibbons considered only three accounts worth reporting.

The best description derives from Lannon Oldenburg, an American gardener cultivating an area near Tupou College. Oldenburg stated that he saw a large lizard (250–280 mm SVL) running along the ground in his plantation. The lizard was dull green with blackish markings dorsally. Tail length was two thirds or more of SVL. The body was not especially thick (ca. 25 mm diameter), and there was no middorsal ridge or crest.

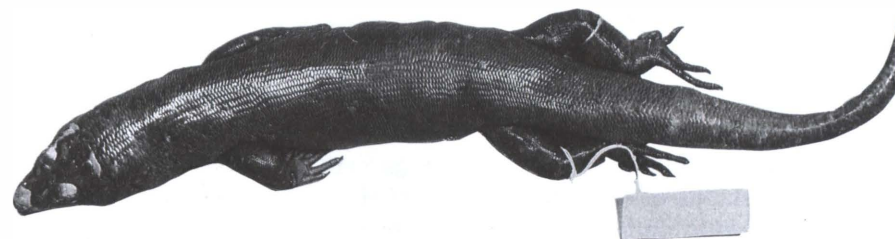


FIG. 2.—One of the two existing specimens of *Tachygia microlepis* located in the National Museum of Natural History, in Paris. MNHN 2919, total length = 323 mm.

A second eyewitness was Peter Chignell, a retired New Zealand headmaster currently assisting with a Tongan prison rehabilitation program. He stated that, during the cutting and burning of high grass and scrub near a small, isolated stand of forest (near the airport, and about 3,000 feet [1 km] from the larger patch of forest noted above), he observed a large lizard. It was greenish with black markings, and about 35 mm in body diameter. It ran rapidly towards the forest patch and away from the fire. Significantly, this small wooded area and the larger surrounding grassy area is much feared and largely avoided by Tongans. The area is avoided because it contains a large, ancient burial mound of about 300 Tongan nobles (Peter Chignell, personal communication to John Gibbons). The sighting of the large lizard took place very close to this mound, and about 150 feet (50 m) from the small woods.

The third account comes from a Tongan student (1986, personal communication to John Gibbons). He stated that a very large lizard or *moko*, not a *fokai* (iguana), is occasionally seen on Tongatapu. If a person sees this huge *moko*, it is a sign that something significant will happen in his or her family, such as a death or a marriage.

The above reports indicate the presence of a large, terrestrial lizard on Tongatapu that is very rare, of restricted distribution, and unknown to most Tongans. Unquestionably, it is not the iguana or *fokai*, as *Brachylophus fasciatus* is highly arboreal, has a distinct middorsal crest, and, although green, lacks the dorsal black markings. Gibbons thought that these three observations indicated the survival of *Tachygia*. We believe the other candidate is the skink, *Emoia trossula*. It is normally olive brown, but many individuals have dark marks on the back; however, *E. trossula* seldom exceeds 160 mm total length. Despite the size discrepancy, *E. trossula* is more likely to be described as running, whereas *Tachygia* would appear to be crawling. According to an examination of the Paris specimens (Fig. 2), *Tachygia* is/was probably dark brown, as are its *Eugongylus* relatives, and without black markings on the back.

DISCUSSION AND CONCLUSIONS

The herpetofauna of Tongatapu is a remnant of its original composition 200 years ago and earlier, especially for the larger lizards. The original forests were largely destroyed by the Polynesians, and now the entire island is covered nearly completely by plantations, gardens, and human housing; only a few small stands of secondary forest remain. Second, domestic cats and rats (*Rattus rattus* and *R. norvegicus*) were introduced by Tasman (1643) and Cook (1770's), and/or by subsequent European expeditions. Detailed accounts of early European contacts within Tonga is given by Langdon (1977). These predators often have a devastating effect on naive native reptiles. Populations of feral domestic cats are now established on virtually every inhabited island in Tonga. A large, ground-dwelling lizard such as *Tachygia* would have experienced heavy and continual predation from these introduced predators.

The authors recently (October 1993) had an opportunity to search for *Tachygia* on 'Eua and Tongatapu. Based on the habits of *Tachygia*'s relatives in New Caledonia and New Guinea, it was a semi-fossorial, forest-floor inhabitant. There are few stands of forest on Tongatapu today. All stands are secondary growth forest, and are highly disturbed by foraging pigs. It seems most unlikely that a semi-fossorial lizard could survive under these circumstances. Therefore, we now agree with Rinke (1986: 150) that the giant Tongan skink is extinct on Tongatapu. However, giant scincoid anguid lizards have managed to survive in the West Indies despite cats, rats, pigs, and mongooses (James D. Lazell, personal communication). If *Tachygia* occurred on 'Eua, sufficient undisturbed forest remains for its continual survival there.

Circumstantial evidence suggests that *Tachygia* was a common food item for the Tongans. La Billardière (1800: 340), during his visit to Tongatapu in 1793, refers to lizards. He stated that Tongans offered captured "*lacerta amboinensis*," and it was very good to eat. Gibbons (1981: 256) assumed that these were the iguana *Brachylophus*. It is also possible that these food-lizards were *Tachygia*. Evidently, La Billardière coined the term "*lacerta amboinensis*" on account of their similarity in shape and size to lizards previously seen on Ambon Island in eastern Indonesia, and to their superficial resemblance to the lacertids of Europe (e.g., *Lacerta viridis* and *L. lepida*). *Hydrosaurus amboinensis*, a big agamid lizard, was originally described as *Lacerta amboinensis* Schlosser, 1768, and this species certainly is more similar to *Brachylophus* than to *Tachygia*. *Brachylophus* was eaten by the early Tongans, but there is no evidence that this was the case for *Tachygia*.

Recent archeological investigations (Pregill and Dye 1989) on Lifuka Island, north of the Tongatapu Group, show that *Brachylophus* was a regular

food item of the Tongans. Pregill and Dye (1989) concluded that the first few hundred years of human colonization had a catastrophic impact on *Brachylophus*, and may have caused a reduction in average body size among the surviving populations. Similar-sized bones were also found in a Lepita midden on Tongatapu Island. Reduced population size by human predation and shrinking habitat would set the stage for extinction by an introduced predator. We cannot conclude that *Tachygia* is extinct, but Gibbons was unable to verify its presence with specimens or unquestionable eyewitness accounts. Nor can we conclude, after our recent investigations, that it survives.

Pregill and Dye (1989:507) stated: "Lizards may now be added to the list of oceanic vertebrates whose large-bodied members have become extinct while smaller-bodied forms survived." They may be correct. The mystery remains.

ACKNOWLEDGMENTS

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THE CRYPTOZOOLOGICAL IMPLICATIONS OF OLD BIRD NAMES IN NATIVE VOCABULARIES

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ABSTRACT: It is now known that the Pacific islands supported many more endemic bird species than exist at present, and that extinction occurred within the past 2,000 years. A comparison of native bird names appearing in a Tahitian-English dictionary published in 1851 with those listed in a modern ornithological work found that, while most of the names in the modern work are included in the 1851 dictionary, there are many names in the 1851 dictionary which are not included in the modern work, and thus do not appear to represent extant Tahitian species. An abstracted list of 55 native bird names from the 1851 dictionary is presented, together with possible identifications. Further collaborative work by linguists, ornithologists, and native naturalists could increase our knowledge of extinct Pacific island birds.

INTRODUCTION

For a long time, it was assumed that no bird species had become extinct on Pacific islands until the discovery and exploitation of those islands by European explorers and colonists, and that, until about the 17th century, all avifaunas were intact and undisturbed. The fact that many 17th, 18th, and 19th century travelogues and faunas, of continental areas as well as islands, contained descriptions of birds that could not be identified with any extant species, was an embarrassment.

This tended to be explained away by the suggestion that the descriptions were erroneous; that they were poor descriptions of something extant (as many have indeed proved to be); that the bird was a hybrid, an artifact, or a freak variety of something else; or that the author was simply a liar. Many descriptions were thus ignored as being irrelevant.

However, evidence has been emerging—much still unpublished (but q.v. Olson and James 1982, Steadman and Kirch 1990)—that many islands of the Pacific Ocean formerly supported many more endemic bird species than at present, that many of those species have become extinct in relatively recent times (i.e., since the Polynesians colonized the area, approximately 2000 years B.P.), and that they almost certainly were exterminated by the native inhabitants. An examination of native bird names and native traditions, therefore, might shed some light on the birds which formerly occurred there.

THE ROLE OF LINGUISTICS

Linguistics and folklore have played important roles in other recent cryptozoological analyses, particularly when examining native vocabularies. Bauer and Russell (1987), for example, have established a linkage between the legendary *kawekaweau*, a reptile of New Zealand Maori folklore, and the giant gecko, *Hoplodactylus delcourti*, a species known from only one specimen of unknown provenance in a French museum. Colarusso (1988), through an in-depth linguistic analysis, has concluded that the supposed *waitoreke* known to the Maoris was (or is) a real animal of mammalian affinities.

Not being a linguist, a proper interpretation of native Tahitian bird names must be left to others. However, I would like to present some linguistic information which I uncovered some years ago when examining a Tahitian-English dictionary published in the mid-19th century (Davies 1851), a publication I discovered by chance in the British Library. The results of my examination were quite astonishing. There were a great many bird names listed, and in comparing them with the local names given in J. C. Thibault's *Birds of Tahiti* (Thibault 1975), it was found that nearly all the names used by the latter author matched those used in the 1851 dictionary, but there were many in the 1851 dictionary which found no counterpart with extant Tahitian species.

Since some birds have alternate names, a certain number of these unidentified names probably represent such alternatives. However, many may actually refer to birds now extinct on the island. An abstracted list appears below. It is arranged alphabetically, and includes identifications based on Thibault (1975).

LIST OF NATIVE TAHITIAN BIRD NAMES

- *Aeto*. An eagle. This name may be of fairly recent introduction, *aetos* being the Greek word for an eagle. Unidentified.
- *Ao*. A large spotted sea bird (apparently this word has a great many other meanings). = *Sula leucogaster*, the Brown Booby.
- *Arevareva* (also called *oovea*). A large spotted bird; said formerly to be inspired at times by the god Manuteaa. = *Eudynamis taitensis*, the Long-tailed Cuckoo.
- *Ataetai*. A white bird, presumably one which runs, swims, or flies speedily, as the word also means nimbleness. Unidentified.
- *Huapareva*. An egg of the bird *pareva* (unidentified), often found on floating rubbish on the sea. Figurative: a person of mean origin or parentage.
- *Huioa*. Name of a bird; also a company prettily attired. Presumably this was a gregarious bird of brightly-colored plumage. Unidentified.
- *Io* or *oio*. The name of a sea bird, from its cry *oio*. Also called *oa*, which

means "to paint black," and the name of the black god of night. = *Anous stolidus*, the Brown Noddy.

- *Itae*. A pigeon. Unidentified.
- *Itatae*. Name of a bird. = *Gygis alba*, the White or Fairy Tern.
- *Mauroa*. A tropicbird. = *Phaethon lepturus*, the White-tailed Tropicbird.
- *Meha*. The name of a small bird. = ?*Nesophylax niger*, Miller's Rail (Walters 1988). The name *maho* given in the original description may be a corruption of *meja*.
- *Meho*. The name of a bird. = *Porzana tabuensis*, the Spotless Crake.
- *Moa*. A fowl. = *Gallus gallus*, the Red Junglefowl. There are a number of other related words:
 - Moafaatito*, a fighting cock.
 - Moahururau*, a fowl of many qualities. Figurative: an unsteady or fickle person.
 - Moa opapa*, a fowl without a tail.
 - Moafiri*, a plant; also a wild fowl.
 - Moapateatoto*, a courageous cock.
 - Moapuruhi*, a cowardly cock.
 - Moataratua*, a cock with a long spur.
 - Moaraupia*, a peculiarly-colored fowl.
- *Moora* or *moore*. A wild duck. = *Anas superciliosa*, the Grey Duck.
- *Moora papaa*. A foreign duck. Unidentified. *Papaa* means a foreigner or white man, so *moora papaa* could refer to *Anas platyrhynchos*, the Mallard, or other ducks which are kept as captives on Tahiti.
- *Moora ao*. A goose. Unidentified.
- *Omamao*. A singing bird "about the size of a sparrow." = *Pomarea nigra*, the Tahiti Flycatcher.
- *Omaomao*. A chattering bird. Its cry is called *oviriviri*. Unidentified.
- *Omaomaopuafau*. Related to the bird *omaomao* that has yellow feathers. Unidentified, but possibly some species of honeyeater (Meliphagidae).
- *Ooea*. Name of a bird. Unidentified.
- *Opea*. Name of a small black bird. Unidentified. This name appears to be used for both *Hirundo tahitica*, the Pacific Swallow, and *Collocalia leucophaea*, the Polynesian Swiftlet.
- *Otaha*. The man-of-war bird. = *Fregata* sp., a frigatebird (two species occur at Tahiti).
- *Otatara*. Name of a bird "resembling the woodpecker." Unidentified. This word appears to be used for both *Halcyon venerata*, the Venerated Kingfisher, and *Acrocephalus caffra*, the Tahiti Reed-Warbler. The cry of the bird *tatare* was *oti* when uttered over a person (when it was supposedly ominous of death). No woodpecker is known ever to have occurred on any of the Pacific Islands.
- *Otuu*. A kind of heron. = *Egretta sacra*, the Eastern Reef Heron.

- *Paetaeta*. Name of a bird. Unidentified.
- *Peheo*. Name of a bird. Unidentified.
- *Perue*. Name of a bird. Unidentified.
- *Petea*. A long-tailed parakeet. Unidentified. This word is apparently now used for *Phaethon lepturus*, the White-tailed Tropicbird, so the original identification could be an error. However, since there is already another name for this species, it is more likely that the name *petea* was transferred to it after the extinction of the parakeet to which the name originally referred (q.v. the case of the name *vini*).
- *Punae*. Name of a bird. Unidentified.
- *Punaneuneu*. Name of a webbed-footed bird. Unidentified.
- *Putii*. Name of a bird. Unidentified.
- *Putoa*. Name of a bird (also of a shell). Unidentified.
- *Putu* (also *oputu*). Name of a bird. Unidentified.
- *Reiamauoa*. Name of a bird with long legs. Figurative: someone who is very fleet of foot; hence: *avaereia*, speedy, with long strides. Unidentified.
- *Rupe*. Name of a large pigeon. = *Ducula aurorae*, the Tahiti Pigeon.
- *Ruro*. Name of a bird formerly sacred to the gods; hence: *hopuhopu-aruro*, "to dip and out again, like the action of the bird *ruro*." Unidentified. It sounds like a probing wader.
- *Ruru*. The name of a large aquatic bird, probably an albatross. Not certainly identified.
- *Ruru*. Another name for *otatare*. = *Halcyon venerata*, the Venerated Kingfisher. Thibault (1975) assigned both names *ruru* and *otatare* to the Venerated Kingfisher, but David Steadman (personal communication) has pointed out that the Respected Kingfisher, *Halcyon tuta*, although now only a rare visitor to Tahiti, occurred there in historical times.
- *Taeifeiaitu*. A bird sacred to the god Tane, one of the principal Tahitian deities. Unidentified.
- *Tapureahuruhuru*. The name of a bird with black and white spots or streaks. Unidentified.
- *Tarahuaru*. Name of a sea bird. Figurative: a talkative, noisy person. Unidentified.
- *Tarara*. Name of a bird. Unidentified.
- *Torea*. Name of a bird. = *Pluvialis dominica*, the Pacific Golden Plover.
- *Toroa*. Name of a sea bird. Unidentified. In the Cook Islands, this name is used for *Sula sula*, the Red-footed Booby (David Steadman, personal communication).
- *Tufaru*. Name of a bird. Unidentified.
- *Uao* or *uaao*. Name of a bird. = *Sula sula*, the Red-footed Booby.
- *Uenia*. Name of a bird. Unidentified.
- *Upaupa*. Name of a mountain bird. Unidentified.
- *Uramarea*. Yellow feathers used "for the gods" (this term is unexplained,

but presumably it refers to some religious ceremony) in the absence of red ones (see *vanevane*). Unidentified.

- *Uriti*. Name of a bird; probably the same as *uriri*, which = *Heteroscelus incanus*, the Wandering Tattler.
- *Uupa*. A kind of pigeon. = *Ptilinopus purpuratus*, Grey-green Fruit-Dove.
- *Vanevane*. Red feathers fastened to pieces of sinnet and "used for the gods" (see *uramarea*). Unidentified.
- *Vini*. Name of a small parakeet. *Vini peruviana*, the Blue Lory, to which the name referred, is now extinct on Tahiti, and the name has been used to refer to *Zosterops lateralis*, the Grey-backed White-eye; *Ramphocelus dimidiatus*, the Crimson-backed Tanager; *Neochmia temporalis*, the Red-browed Firetail; *Estrilda astrild*, the Common Waxbill; and *Lonchura castaneothorax*, the Chestnut-breasted Mannikin, all of which have been introduced since 1851. Clearly, it has become a general term for a small passerine.
- *Vinipaura*. A species of *Vini*, of which there are several varieties distinguished by color:
 - *Vinipauri* (paure = black or dark).
 - *Vinitea* (tea = white).
 - *Vinitete* (tete = ?).
 - *Vinirehu*. (rehu = ashes? = grey).
 - *Vinipapaa*. Apparently means foreign.

Some of these may refer to plumages of *Vini peruviana*, the Blue Lory, or to the extinct species *Vini vidivici* and *Vini sinotoi*, both of which are recorded archaeologically from Huahine.

- *Vinitunupaa*. Roasted *vini*(!), but also signifying something rare and delicious. This suggests that the Blue Lory was eaten by the Tahitians, and this may have been the cause of its extinction.

That Tahitian bird names were probably not static, and that there may have been even more of them than here outlined is suggested by several names used by Thibault (1975) which do not appear in the 1851 dictionary:

- *Noha*. = *Pterodroma rostrata*, the Tahiti Petrel.
- *Hao*. = *Butorides striatus*, the Mangrove Heron.
- *Manu'amu moa*. = *Circus aeruginosus approximans*, the Swamp Harrier. This subspecies was introduced to the island in 1885.
- *Teu'e* or *kivi*. = *Numenius tahitiensis*, the Bristle-thighed Curlew.
- *Tarapapa*. = *Sterna bergii*, the Swift Tern.

DISCUSSION

Fieldwork by Storrs Olson and David Steadman has revealed bird bones on many Pacific islands which refer to species now totally extinct. On some islands, the avifauna appears to have been positively devastated, presumably by the native inhabitants. While the work of uncovering bones continues, much valuable information might be uncovered by the study of bird names

in native island languages. These studies would benefit from the corroborative efforts of linguists, ornithologists, and native naturalists.

Native words, like fossils, often survive to modern times, and may be used by investigators to further our understanding of past faunas. That is why linguistic research is important in many areas of natural history, including cryptozoology. Coupled with paleontological and archaeological discoveries, linguistic research can help researchers learn more about species that have vanished, and also about those that may persist cryptically to the present.

ACKNOWLEDGMENTS

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(Editor's Note: Normally, the English vernacular [common] names of species appear uncanceled [lower case] in zoological journals. However, by convention, ornithologists, who designate very specific vernacular names to all bird species, use capitalization in their published works. This results in style inconsistencies in non-ornithological journals, particularly when the vernacular names of bird species and other vertebrate species appear in the same paper, with bird names capitalized but the others not. After consulting with several zoologists, including ornithologists, the following rule has been adopted for Cryptozoology by the Editor: if a paper deals exclusively or almost exclusively with birds, all vernacular species names will be capitalized, following ornithological convention; if a paper deals mainly with other vertebrate forms [fish, amphibians, reptiles, or mammals], or even with invertebrates—in which birds are mentioned only in passing—all vernacular species names, including those of birds, will not be capitalized—Editor.)

Research Reports

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MOLECULAR GENETIC IDENTIFICATION OF A MEXICAN ONZA SPECIMEN AS A PUMA (*PUMA CONCOLOR*)

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ABSTRACT: Tissue samples from an alleged Mexican Onza, shot in the western Sierra Madre in 1986, were subjected to several biochemical assays in an attempt to determine the specimen's relationship to felid species of North America. Protein analyses included isoenzyme electrophoresis and albumin isoelectric focusing. Mitochondrial DNA was assayed for restriction fragment lengths with 28 restriction enzymes, and the ND5 gene was sequenced. The resulting protein and mitochondrial DNA characteristics of the Onza were indistinguishable from those of North American pumas.

INTRODUCTION

In the Americas, there are two documented species of large cats: 1) *Puma concolor*, the puma, also called mountain lion, cougar, and panther in different regions of North America, and known as *leon* in Mexico; and 2) *Panthera onca*, the jaguar, or *tigre* as it is known south of the U.S. border.

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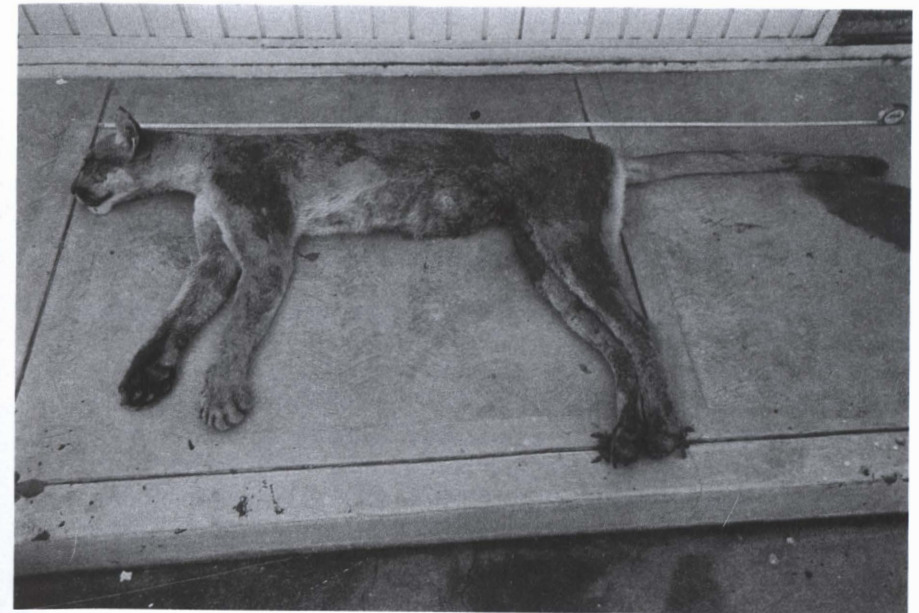


FIG. 1.—Onza shot in 1986 in western Sierra Madre, Mexico. (International Society of Cryptozoology.)

But in the western Sierra Madre of Mexico, locals speak of three species: *leones*, *tigres*, and *onzas*.

References to a third big cat in the Americas date from 1519 when Bernal Diaz del Castillo, a member of Cortez's conquering army, visited Montezuma's palace, which included a zoo (Greenwell 1987). Diaz del Castillo wrote of seeing "tigers" as well as "lions" of two kinds. Jesuit missionaries of the 18th century also described a cat that was longer and leaner than the puma. Modern hunters say it has the coat color of a puma, a leaner body, longer legs, and a very aggressive nature. Interest in the Onza was rekindled in the 1960's with the publication of a book by Arizona hunter Robert Marshall (1961).

On January 1, 1986, Andres Rodriguez Murillo, a rancher in the state of Sinaloa, northwestern Mexico, shot a cat at night which he at first thought to be a jaguar. Upon examination, he discovered that it was not a jaguar, and seemed different from a puma (Greenwell 1986, 1987). Rodriguez remembered a recent visit to the area by two Americans (Richard Greenwell and Robert Marshall) looking for unusual big cats. He took the cat to a neighbor, Manuel Vega, who, upon examining the animal, immediately identified it as an Onza. The animal had the coloration of a puma, was lean, and had long legs. Vega contacted Ricardo Urquijo, a well-known rancher and an associate of Greenwell, who immediately had the specimen placed in a

fisheries cold storage in Mazatlan. This was reportedly the first complete specimen of a supposed Onza collected in over a decade (Fig. 1).

Greenwell, University of New Mexico mammalogist Troy Best, and graduate student Ned Gentz flew to Mazatlan in February, 1986, and examined the animal at the Regional Diagnostic Laboratory of Animal Pathology of the Mexican Ministry of Agriculture. The specimen was dissected, and tissue samples were removed, preserved, and brought back to the U.S. for analysis.

METHODS

Frozen tissue samples (liver, heart, and blood) from this specimen were sent to Stephen J. O'Brien's laboratory at the National Cancer Institute by Best and Greenwell. DNA was extracted from the heart sample (Sambrook 1989), and was included in a mitochondrial DNA restriction fragment analysis with nine other cat species: lion, tiger, leopard, snow leopard, clouded leopard, jaguar, puma, cheetah, and bobcat. DNA from several individuals of each species ($n = 2-10$) was used for the analysis. Twenty-eight restriction enzymes were used; southern blots were made and hybridized with a radioactively-labelled molecular clone of domestic cat mitochondrial DNA (O'Brien et al. 1990, Menotti-Raymond and O'Brien 1993).

The mitochondrial NADH dehydrogenase subunit-5 gene (ND5) was amplified by polymerase chain reaction (PCR; Mullis and Faloona 1987) for 30 cycles, using 2.0mM MgCl₂ and 49°C annealing temperature, with primers MCND5-1 and MCND5-2 modified by the addition of M13 tails for sequencing. The resulting PCR products were prepared for sequencing using the PRISM dye primer procedure (Applied Biosystems, Inc.). Sequencing was done in the ABI 373A DNA Sequencer.

For isoenzyme electrophoresis, crude protein extracts were made, and isoenzyme electrophoresis was performed according to published protocols (O'Brien 1980, Newman et al. 1985). Isoelectric focusing of tissue homogenates was performed on the Pharmacia PhastGel system using precast gradient gels reconstituted in urea for pH range 5-6 (Hoesch and Dratch 1992), followed by Western blotting using anti-bovine serum albumin (Towbin, Straehelin, and Gordon 1979).

RESULTS

Mitochondrial DNA (mtDNA) is a cytoplasmic chromosome of approximately 17,000 nucleotides that has proven very useful in forensic and genetic identification studies (Avisé 1994, Baker et al. 1993, Menotti-Raymond and O'Brien 1993, O'Brien et al. 1990). By digesting mtDNA with several restriction enzymes, each of which recognizes a unique 4-6 nucleotide sequence when it occurs in the mtDNA sequence, a series of "restriction" fragments are identified which are unique to each species. They can be diagnostic for subspecies as well. Individuals of the same species share a large fraction (95-

TABLE 1.—Mitochondrial DNA fragment sharing with Onza specimen.

Species comparison	Total restriction fragments scored	Number restriction fragments shared	Percent restriction fragments shared
Onza : cheetah	120	34	28.3%
Onza : jaguar	135	24	17.7%
Onza : puma	122	122	100.0%
lion : leopard	148	54	36.4%
lion : puma	131	28	21.3%
lion : jaguar	155	36	23.2%
lion : tiger	158	52	32.9%
tiger : snow leopard	143	60	41.9%

100%) of mtDNA restriction fragments, while individuals of distinct species share fewer identical fragments, largely as a consequence of mutational divergence over time.

The mtDNA analysis revealed between 120 and 158 mtDNA restriction fragments for each of the felid species involved. An example of the comparison of several species' mtDNA restriction fragment patterns is illustrated below (Fig. 2). Pairwise comparisons of the frequency of restriction fragments shared between species are presented in Table 1.

These results reveal a perfect identity between the mitochondrial DNA restriction fragment pattern of the Onza sample and four North American pumas (*P. concolor*) used in the study. The next closest relationship was between a tiger and a snow leopard (41.9%), followed by lion : leopard (36.4%), and lion : tiger (32.9%).

We chose to sequence the ND5 gene, which is known to be one of the more rapidly evolving genes in the mitochondrial genome (Lopez et al. in preparation), and, therefore, most likely to present sequence polymorphisms between closely related individuals. DNA fragments of 245 base pairs of the ND5 gene were generated by PCR from the Onza DNA, from more than 50 western North American pumas (including two from southern Mexico), from one jaguar, and from one cheetah. The nucleotide sequences were then compared. Fig. 3 shows the alignment of ND5 sequences (all the western North American puma sequences were identical, so only one puma sequence is shown). The Onza sequence was 100% identical to the western North American puma sequence, 87% similar to the cheetah, and 83% similar to the jaguar. Cheetah and jaguar sequences were 85% similar.

Since mitochondrial DNA is maternally inherited, the possibility still existed that the Onza specimen was a hybrid between a female puma and a male jaguar. However, several studies of nuclear-DNA encoded proteins and isoenzymes indicated this was unlikely. Researchers at Texas Tech University had earlier compared the electrophoretic mobilities of 19 isoenzymes, and found no differences between the Onza sample and west Texas pumas

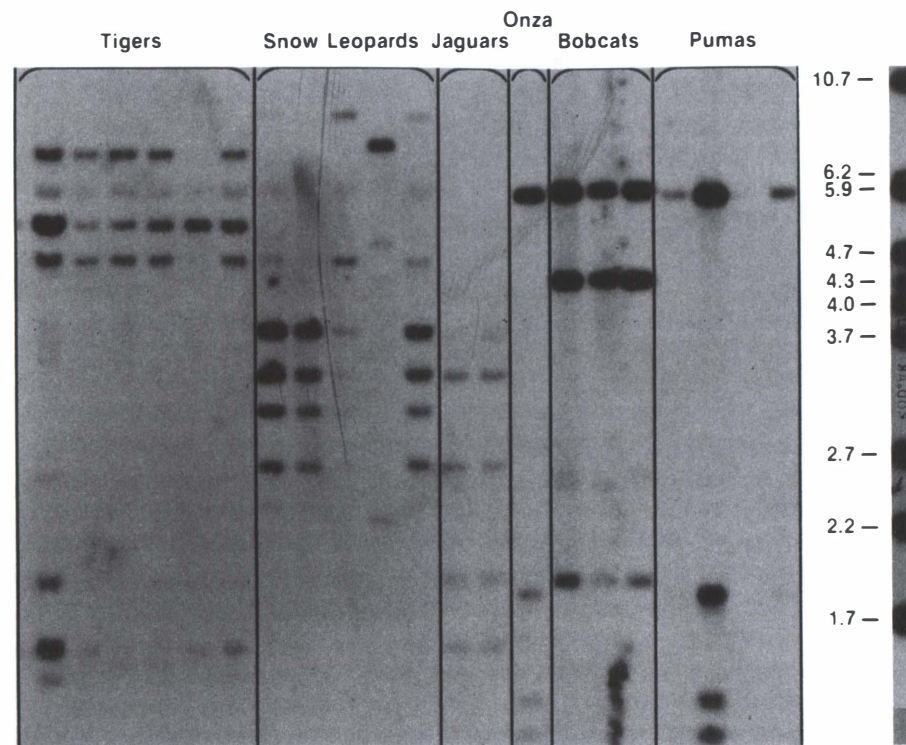


FIG. 2.—Restriction Fragment Comparison: Autoradiograph of mitochondrial DNA fragments produced with the restriction enzyme *AccI*, one of 28 restriction enzymes used in the band sharing analysis. Species are as indicated. Note intraspecific variation in snow leopards. Molecular weight markers (BamHI/EcoRI digest of adenovirus II DNA) are in the right lane; sizes are in kilobases.

(Richard Greenwell, personal communication). Prior to receiving the Onza sample, an isozyme study performed at the National Cancer Institute had revealed mobility differences for 16 isoenzymes between Texas pumas and jaguars (Stephen O'Brien and Janice Martenson, unpublished observations). If the Onza was a puma \times jaguar hybrid, isoenzyme electrophoretic differences (e.g., hybrid protein molecules) would have been detected in the Onza sample by the Texas Tech researchers.

Isoelectric focusing on narrow range urea gels (pH 5–6) and immunoblotting for albumin showed that the Onza had a banding pattern consistent with 40 puma standards, and inconsistent with five jaguar and three cheetah standards (Fig. 4). The albumin isoelectric point of the Onza sample was also different from that of all other cat species analyzed: lion, tiger, snow leopard, clouded leopard, as well as Geoffroy's cat, jaguarundi, serval, fishing cat, lynx, bobcat, and domestic cat.

puma	ATTCTACTTC	TACCTATCAT	CATATCCAAT	ACCCAATTAT	ACAAAAACAG	TCTGTATCCC
onza
cheetah	..C....ATC....T..CG....	.T...GG..A	CT.A..C...
jaguarC.C....	T.C.....C	..T...C....	.T.....A	C.....C..T
puma	TACTATGTAA	AAACTACAGT	CTCCTACGCC	TTCACCATCA	GCATAATCCC	AACCATAATA
onza
cheetah	C.....C...A.A.....T....
jaguar	C.....C...A.T..T...	..T.....T..	GG.T...G..
puma	TTTATCTCTT	CAGGGCAGGA	AGCAGTCATC	TCAAAGTGAC	ACTGACTGTC	AATTCAAACC
onza
cheetahT..A....	..A.....T.A..C.....
jaguarT....	..C..A....	..A..A.T...T.A..C.....
puma	CTTAAAGTGT	CATTAAAGCTT	TAAAGATAGAC	TACTTCTCAA	TCATCTTTAT	CCCCGTAGCA
onza
cheetahT..A.	..C.....T....C..C
jaguarT...	..C....T..C.....	..T....G.C..T.....
puma	CTATT					
onza					
cheetah					
jaguar	..T..					

FIG. 3.—Alignment of ND5 nucleotide sequences from puma, Onza, cheetah, and jaguar. The dots indicate nucleotide identity to the puma sequence.

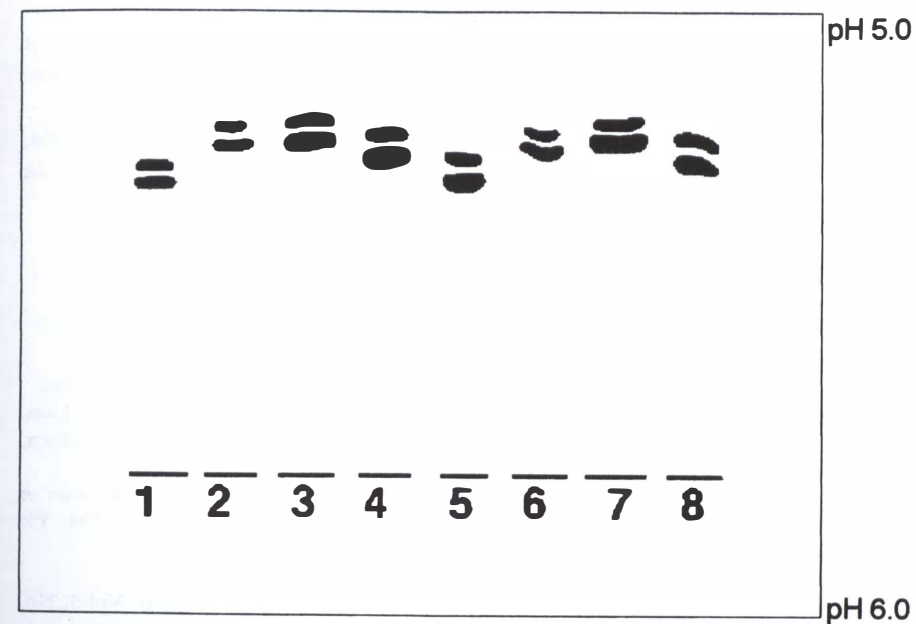


FIG. 4.—Scan of an immunoblot of albumins on narrow range isoelectric focusing gel. Lanes 1 and 5 are cheetah samples, lanes 2 and 6 are puma, lanes 3 and 7 are the Onza, and lanes 4 and 8 are jaguar.

DISCUSSION

Several hypotheses have been proposed on the taxonomy of the Onza (Greenwell 1987): 1) it is a new species closely related to the puma; 2) it is a subspecies of puma distinct from the puma subspecies already described in the region, *Puma concolor azteca*; 3) it is a hybrid between puma and jaguar; 4) as proposed by Helmut Hemmer, it represents a relict population of the Pleistocene North American cheetah, *Acinonyx trumani*, described by paleontologist Daniel Adams and believed to be extinct (Adams 1979, Orr 1969); and 5) it is a puma, adapted to the particular environment of the western Sierra Madre, or perhaps emaciated.

Greenwell's examination of *A. trumani* skulls demonstrated that the 1986 Onza specimen, at least, was not a Pleistocene cheetah (Greenwell 1986), eliminating hypothesis four. The albumin isoelectric focusing and the isoenzyme data shows that the Onza specimen was indistinguishable from North American puma samples, and presented no evidence of hybridization, effectively eliminating hypotheses one, two, and three. The mitochondrial DNA studies revealed perfect identity between the mtDNA of the Onza specimen and western North American pumas, again eliminating hypotheses one and two.

Hypothesis five is best supported by our observations that the DNA and proteins obtained from this Onza sample had molecular characteristics indistinguishable from those of western North American pumas. Based upon these cumulative data, we must conclude that the 1986 Onza specimen was a puma, and did not represent a distinct, new, or relict species.

We thank the International Society of Cryptozoology, Tucson, Arizona, and in particular Troy L. Best and J. Richard Greenwell, for providing Onza tissues, photographs, and relevant information.

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Field Reports

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FURTHER INVESTIGATIONS INTO UNKNOWN PERUVIAN MAMMALS

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INTRODUCTION

For many years, I have been collecting reports of several large, zoologically unknown mammals from the native peoples of the Peruvian Amazon. My living in Peru, traveling widely throughout the country, and visiting remote areas in connection with both my zoological and missionary work, has greatly facilitated my learning about such cryptic animals from native hunters.

In a previous article (Peter J. Hocking, 1992, *Large Peruvian Mammals Unknown to Zoology*, *Cryptozoology*, Vol. 11: 38–50), I discussed several of these cryptids, three being large felids which I have tentatively named the giant black panther, the speckled tiger, and the striped tiger. Another large mammal I discussed was an unknown arboreal primate known as the Isnachi. During 1985–1986, I undertook collecting expeditions in the Contamana Mountains (Loreto province—or “department,” as provinces are known in Peru) hoping to find this mysterious ape-like monkey, only to find that it was not known there, but was known in other isolated tropical mountain ranges. Thus, in 1988, I led an expedition into the Orellana Mountains, another remote range in the same general region, but at a higher elevation.

I had already received several reports of the Isnachi being killed in the Orellana Mountains. However, the expedition was not able to reach the higher elevations, and it became clear that the location was simply too far

from regular travel routes—such as rivers, roads, and trails—to be accessible in the time I had available. Thus, I decided to conduct future efforts in the more accessible areas where the primate reportedly occurs; that is, in the Yanachaga Mountains of the Yanachaga National Park (Pasco province). Upon investigating as to what work had been carried out previously in that National Park, which was relatively new, I was surprised to find that its wildlife had never been seriously studied.

I was also interested in the Yanachaga Mountains because the three large felid cryptids I discussed earlier (Hocking, 1992, above) are reported in the same region. Thus, in 1991, I turned my attention to this particular tropical range in my continuing search for evidence of the Isnachi.

The Yanachaga National Park was officially established in 1986. It is not a large reserve. It is located in central Peru in Cerro de Pasco province (Fig. 1). Most of the Park encompasses montane forest, but some lowland forest also occurs, and even *paramo* (*puna*) at an altitude of about 13,000 feet (4,000 m). The Park can be entered from the Oxapampa-Huancabamba valley to the west, near the highest peaks of the range, which reach an altitude of 13,000 feet (4,000 m), or from the Palcazu valley to the east, which represents the lowest part of the range at only 1,000 feet (300 m) altitude (see Fig. 1).

Both valleys have dirt roads linking them to the coast; these are used daily by buses, large trucks, and pick-up trucks. The road to Oxapampa is the best. Austrian settlers and Quechua peoples populate the Oxapampa valley, while the Palcazu valley is populated primarily by Yanesha natives. The Yanesha are peaceful and bilingual.

In 1992 and 1993 I led two expeditions into the region. This report covers the results of both expeditions.

NARRATIVE DESCRIPTION

First Expedition: July, 1992. On this expedition, our primary cryptozoological objectives were to gather information as to the best place in the Park to search for the Isnachi, and to locate good native hunters who could help me collect a specimen.

The team consisted of three naturalists from the Natural History Museum of the National Higher University of San Marcos, in Lima, Daniel Hocking—my 19-year-old son—and myself. (My son, who is a budding naturalist, helped me with skinning bird specimens.) We traveled overland from Lima, the capital, to the jungle town of Villa Rica, a 12-hour trip. From there, we traveled for five hours by pick-up truck to the Yanesha village of Shirin-gamazu. There we hired five native helpers to serve as porters and hunters, and also a cook. We carried out this expedition from the Palcazu valley, rather than entering the Park directly through the usual route of the Paujil

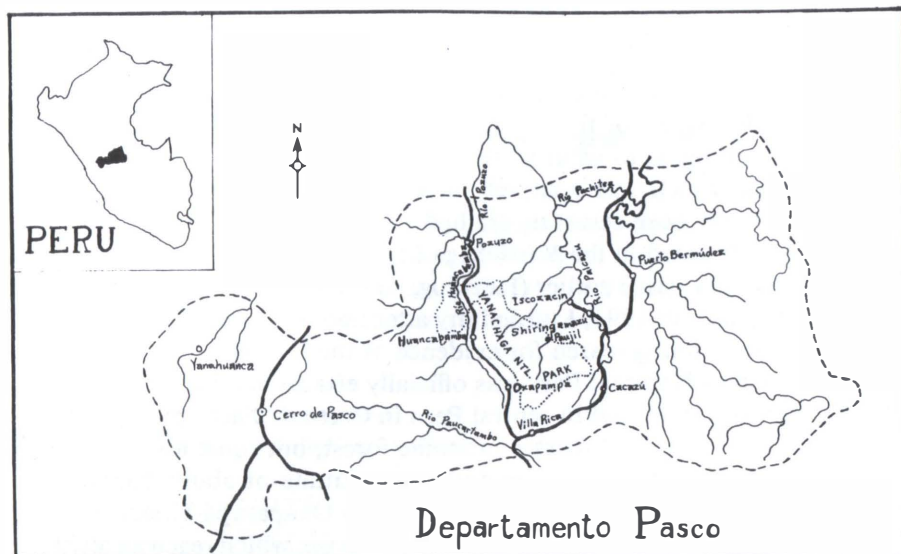


FIG. 1.—Generalized map of Pasco province (“department”) showing location of the Yanachaga National Park (dotted line), where the author conducted the two expeditions described in this report.

Biological Station on the upper River Iscozacín. We headed for the Park limits from Shiringamazu (see Fig. 1), and then started on foot for the top of the high ridge of Cerro Chontilla.

This proved to be the most difficult part of the expedition, as we had to climb uphill for six hours, carrying heavy loads from 1,000 feet (300 m) to almost 3,200 feet (1,000 m). Those of us from Lima had serious doubts about making it, but in the end we reached the summit of Cerro Chontilla (Fig. 2). It was well worth the effort, for we found great biodiversity in the area, and collected several valuable bird and small mammal specimens.

After a week of collecting specimens, we descended down the western side of the ridge to a stream called Mucniz. Camping for a week at the farm of one Jose Roca, we collected more birds and mammals. In the final stage of the expedition, we climbed the next ridge, Cerro Jonatan, making camp at the summit, at an altitude of 2,100 feet (650 m). This placed us at the edge of the Park. After collecting for another week, we returned to Lima, unable to penetrate into the Park itself for lack of time. Nevertheless, we had been able to acquire valuable zoological specimens, and gather further information on crypto-mammals.

Second Expedition: September–October, 1993. The main objective of this expedition was to look for the Isnachi and the unknown felids. On this occasion, the work was carried out by two separate groups. My son, two



FIG. 2.—The tropical cloud forest of the Yanachaga Mountains, as seen from Cerro Chontilla.

native Yanesha helpers, and myself formed the first group. We collected specimens for two weeks in late September. The second group consisted of four naturalists from the Museum, but without native helpers (only the help of the Park rangers). They collected specimens during two weeks in October. As before, we approached the Park from the Palcazu valley in the east, but this time taking the route of the Iscozacín River and the Paujil Biological Station.

Both teams followed the same plan. As before, the first stage of our journey was from Lima to Villa Rica by bus. On the next stage, through the Palcazu valley by pick-up truck, we met up with our two native hunter-helpers. After a six-hour trip, we reached the end of the road at the town of Iscozacín. There, we boarded a large motorized Park canoe, and were taken by Park rangers up the Iscozacín River. We encountered rapids and other difficulties, but after three hours we finally reached Paujil, at the edge of the Park (see Fig. 1).

We collected specimens near the Biological Station, and also further into the Park, where animals were abundant and unafraid of humans. However, we found no sign of the Isnachi. We were now in “tiger” country, and although the Park rangers were threatened one entire night by a black jaguar, *Panthera onca*, my team was unable to observe or attract a single felid, even though bait was set for them at three different places. Nevertheless, both

teams collected several valuable bird and bat specimens, and I obtained more information on cryptic mammals and a bird which seems zoologically unknown.

RESULTS

The results of this fieldwork will be summarized in separate parts, as follows:

The Isnachi. Although we did not encounter this mystery primate (see Hocking, 1992, above), a native hunter confirmed to me that it exists in the Yanachaga Mountains. He is an old Yanesha hunter living in Shiringamazu. I could tell that the other men in the village respected him greatly for his expertise as a hunter, and for his knowledge of the region's wildlife. He agreed to lead me to the place near the Pescado River where he said the Isnachi can be found, but he was unable to do so at the time of either expedition because of family responsibilities.

The Speckled Tiger. In 1992, I received a report of a speckled tiger (see Hocking, 1992, above) being killed that year in the Villa Rica region. When my team stayed in Villa Rica on our way back to Lima, I talked with the man reported to have killed the cat, a schoolteacher named Isaac Carhuamaca Hinojosa. He stated that, earlier that year, he and a native Ashanica had been hunting on a large, forested, pyramid-shaped mountain—which is in sight of Villa Rica—near the headwaters of the Bocaz River, when they surprised a large speckled tiger that threatened them. The native killed it with his shotgun.

Carhuamaca described the cat as light gray with solid-black speckles, similar to the previous description I had received of this peculiar felid (see Hocking, 1992, above). After the cat was skinned, he took the skin with him, but left the body with the native for food. After drying the skin, he took it to the town of Huancayo for safe-keeping. It was being kept there by relatives. I offered him a good price for the skin and skull, and provided him with all the necessary information for sending them to me in Lima. Although he promised to send me the skin with his wife, nothing came of it.

Later that year, I learned through friends that, when his wife went to pick up the skin in Huancayo, she found that the relatives had sold it, and that no one knew where it was. Naturally, this turned out to be a frustrating experience for me—coming so close to acquiring the skin of what appeared to be an unknown species of large felid, and then not being able to do so.

In 1993, when I passed through Villa Rica again on the second expedition, I begged the schoolteacher to do everything possible to obtain the skull for me, informing him that I would pay him well. Unfortunately nothing came of that either.

The Striped Tiger. During the first (1992) expedition, I was informed that

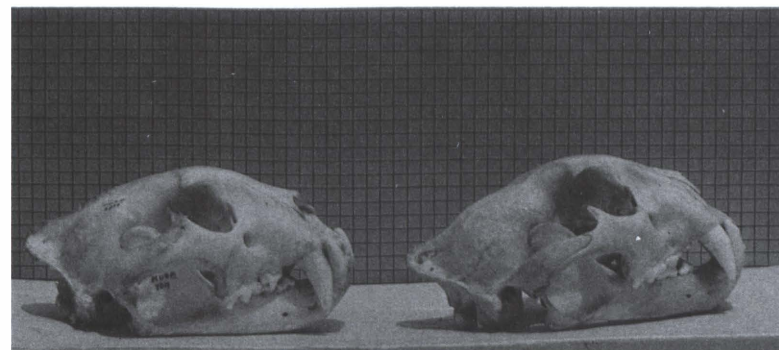


FIG. 3.—The skull of a striped tiger (right), compared to that of a jaguar, *Panthera onca*. This skull may represent the first solid evidence that a large, zoologically unknown felid species occurs in the Peruvian Amazon.

a striped tiger (see Hocking, 1992, above) had reportedly been killed by a rancher near Puerto Bermudez, to the east of our location. Apparently the cat had been killing his cattle. However, nobody knew what had happened to the skin.

Several months later, I did acquire the skull of what was reported to be a striped tiger. I was in my home in Lima when I received a telephone call from a friend who had come to the city on business. He said that he had an unusual “tiger” skull for me. He lives in Oxapampa, near the highest Yanachaga peak. Years earlier, I had asked him to be on the lookout for unknown felids in his region.

I went to his hotel, and he handed me a skull he had obtained from a hunter in the Pozuzo region (Pasco province). The hunter had told him that it had come from a striped tiger which he had killed. Although the hunter had not had an accompanying skin to prove his claim—he had already sold it—my friend had bought the skull anyway. I noticed that it had been carefully cleaned, as if it were something special.

Upon comparing the skull to jaguar skulls at the Museum, I found some interesting differences. Since then, I have sent photographs of the skull to felid specialist Steven C. Conkling, a paleontologist affiliated with the Natural History Museum of Los Angeles County, in the U.S. He has confirmed to me that a number of anatomical features seem to distinguish the skull from jaguar skulls, and that the evidence warrants further careful study to determine if a new species is involved. Official paperwork is currently being prepared to send the skull to Dr. Conkling on a museum-to-museum loan. This skull (Fig. 3) may well represent the first solid evidence that a large, zoologically unknown felid species occurs in the Peruvian Amazon.

Jungle Lion. I received several reports of what I have tentatively called a

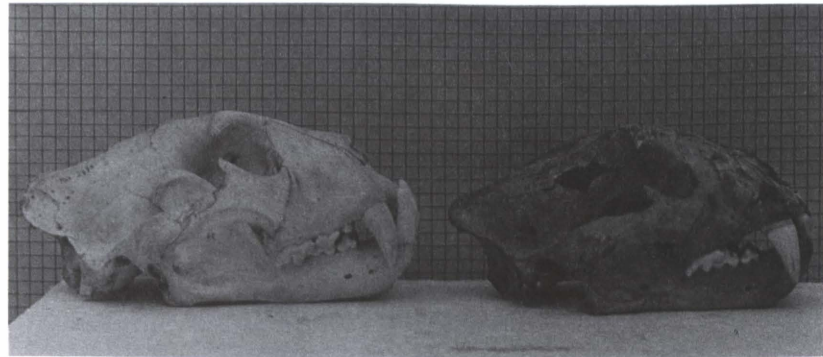


FIG. 4.—The skull of an anomalous jaguar reported to have leopard-like spots (right), compared to that of an ordinary jaguar, *Panthera onca*.

“jungle lion.” I had heard stories of this supposed felid previously, but had not given them much credence. This cryptid is reported to be large, the size of an African lion, with long hair around its neck, but shorter than that of the African lion. Its color is said to be entirely reddish-brown. Rangers have reported seeing this felid in the Park, and one of my native helpers claimed to have once examined a dead specimen killed outside the Park, near Shir-ingamazu. I shall provide further information on this reported felid in a future paper.

Anomalous Jaguar. In 1993, I obtained the skull of another strange felid. It had been shot that same year by one of the native hunters who had worked for me during that year’s expedition. He had killed the animal outside of the Park. Since it had been badly shot up, and—because of the heavy spotting—seemed to be a kind of jaguar, *Panthera onca*, his hunting companion sold the skin to a traveling merchant. Thus, I was only able to acquire the skull, which shows some differences from ordinary jaguar skulls (Fig. 4). This evidence is also being evaluated by Steven C. Conkling.

The hunter, who also skinned the animal, described it to me carefully. The felid was the size of a jaguar, but it was covered with solid-black irregular spots, rather than the normal jaguar-like rosettes and spots. Its background color was cinnamon-brown and white, rather than the jaguar-like yellow and white. In some ways, the cat’s skin may have looked like that of the Afro-Asian leopard, which, of course, does not occur in South America. This report was a complete surprise to me, as well as to the native hunter. Yanesha hunters of the region know the wildlife very well, but none had previously reported such a cat. I hope to provide more information on this leopard-like, anomalous jaguar in a future article.

Pygmy Brown Bear. I also received several curious reports of a pygmy brown bear, an animal I had never even heard of before. It supposedly lives

in the Park, as does the well-known spectacled bear, *Tremarctus ornatus*. Park rangers claim to have seen the bear several times, including a young one being raised by a local person. The spectacled bear is the only known living ursid in South America, so these reports are particularly interesting. I will also provide further information on this cryptid in a future article.

Black Guan. Through the Park rangers, I also learned of a form of bird quite new to me, and, I believe, unknown to ornithology. It is a guan that is entirely black, without a fleshy wattle, such as is found on the black guan *Aburri aburria*. The known black guan also lives in the Park, but at lower elevations. This possible new species will be included in a future paper I am preparing on unknown birds in Peru.

In summary, according to the information I have been collecting over the years, there may be several zoologically unknown species of large felids living within or near the Yanachaga National Park, as well as the mysterious Isnachi monkey, and perhaps a new species of bear.

FUTURE PLANS

I plan to continue carrying out expeditions in the Yanachaga National Park searching for evidence of the Isnachi and the unknown felids. These expeditions will be annual or bi-annual. I will continue to report my findings in future issues of this journal.

Eventually, I hope to collect enough evidence to prove the existence of at least some of these cryptids, which would lead to new species descriptions. The cooperation of other zoologists in this quest would be welcomed. If the cryptozoological evidence I have collected turns out to be accurate, and we are successful in our efforts, the Yanachaga National Park could become famous as the abode of the greatest number of large new mammal species anywhere in the world.

OBSERVATION OF A YETI IN THE HIMALAYAS OF TIBET

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(Translated by Dmitri Bayanov)

INTRODUCTION

In September of 1991, in the Himalayas, the author observed and photographed, at a considerable distance, a human-like animal which may have been a Yeti. As a specialist in biogeography and a biologist by training, the author is not disposed to reaching a definitive conclusion regarding the nature of the observed animal, but he thinks the incident warrants publication of this report.

As a member of the (then) Soviet-Chinese Glaciological Expedition, the author worked in Tibet for three months, from August to October, 1991, visiting many regions of the Tibetan plateau and the Himalayas, including the environs of Mounts Qomolungma (Everest), Xixiabangma, Choja, and several others. It was on one of these routes that the author had the Yeti encounter described below.

NARRATIVE DESCRIPTION

In September, 1991, the Expedition worked on the northern, southern, and southeastern slopes of Mt. Xixiabangma. On the day of the encounter, our small group had worked in a spot some 25 miles (40 km) from the settlement of Nyalam, on the Nepal-China border (Fig. 1). The group consisted of four scientists from the Commonwealth of Independent States (CIS), five specialists from China, and two Tibetan yak drivers. We worked at altitudes of 15,700–16,700 feet (4,800–5,100 m) above sea-level, in a transition zone between high-elevation steppes and the glaciers; this zone is interspersed with shrub growths and rock debris.

The continental montane climate of the region is characterized by cool summers with precipitation, and cold, snowless winters. From November to April, there is practically no precipitation. The rainy season lasts from July to September, when temperatures rise to 50–59°F. (10–15°C.) in the daytime, and fall to 21°F. (–6°C.) at night. Forests occur some 30 miles (50 km) from the area, at altitudes below 12,500 feet (3,800 m).

On September 22, 1991, I was en route to a glacier on the southeastern slope of Mt. Xixiabangma. About midday, on the top of a moraine ridge at

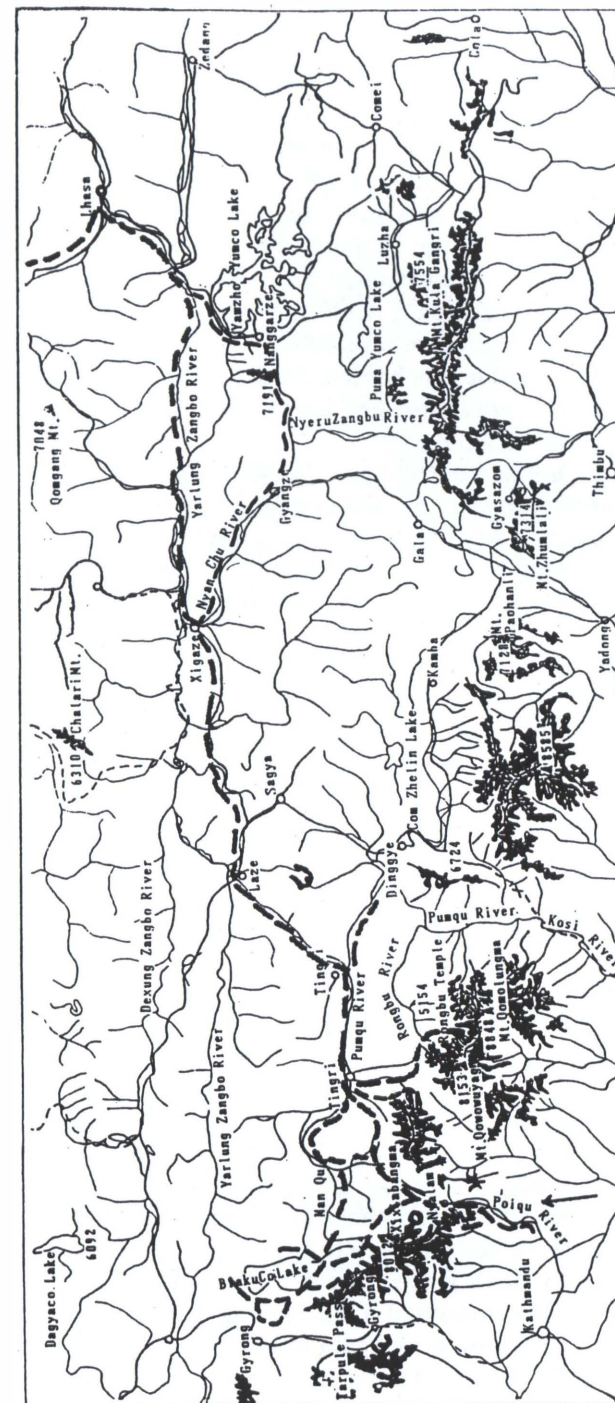


Fig. 1. — Map of routes and fieldwork of the Soviet-Chinese Glaciological Expedition, Tibet, 1991. Circle (lower left) indicates the site of Yeti encounter.

a distance of about 400 feet (120 m), I noticed a human-like animal sitting by a boulder on the sunlit side (Fig. 2). My position was lower on the slope, and at first I did not see the full silhouette. A little later, however, I observed the animal in full. It had the following characteristics: erect, bipedal posture; dark brown color; cone-shaped head; no visible neck; long forelimbs; and short and slightly flexed hindlimbs. When first observed, the creature was squatting in what seemed an unnatural position for an animal, with its back touching the sun-warmed surface of the boulder. It was a rare moment for this altitude and season, when sunshine floods the mountains, and nature comes alive after a cold morning. I could not identify what I saw. My very first impression was of a creature resembling a monkey, a dog, or a bear. But its anatomy, its subsequent behavior, the great absolute elevation of the place, and the distance from forests and settlements made me conclude that I faced a different kind of animal.

Tempted by the natural interest of a biologist, I bent down and started stalking the creature, skirting the boulder on the left-hand side (Fig. 3). However, the attempt proved unsuccessful: the noises I made apparently startled it, as it appeared from behind the boulder on two feet and, slightly bending and helping itself along with a fore-limb, moved behind another (and bigger) boulder. I do not think it saw me, and presumably only reacted to the noises I had made.

I then remembered that I had a camera in my bag, a Soviet-made Zenit-TTL (with no telephoto lens), and that an attempt could be made at photographing the animal. After adjusting the camera to the right setting, I began to approach the boulder. The intended maneuver remained the same: rounding the boulder from a position somewhat lower on the slope, and observing the creature from a closer range. I tried unsuccessfully to take the first photo from a distance of 330 feet (100 m)—maybe more—at a moment when the sun appeared from behind a cloud. To see the whole of the animal, I straightened up, but the effect of photographing from “below” (see Fig. 3) was bound to distort the image. When its full silhouette appeared in the viewfinder, the animal turned its head toward me and began watching me. I then became frightened, and dared not even move at first. Finally, I turned and ran down the slope.

Because of the high altitude, I quickly tired, having covered no more than 1,000 feet (300 m). My colleagues were at a still lower elevation, walking along a stream. They did not react to my yells and arm-waving because of the noise of the stream. To descend and cross the stream to join them would have been quite an effort, and contrary to my objective, which was to reach some moraines near a small glacier about 6 miles (10 km) from the spot where I encountered the presumed Yeti. Having assessed the situation, I decided to return and attempt once again to photograph the animal from a closer range. For this purpose, I took off my bright-colored jacket, and,



FIG. 2. — View of the landscape at Yeti encounter site. Arrow (center) indicates precise encounter location.

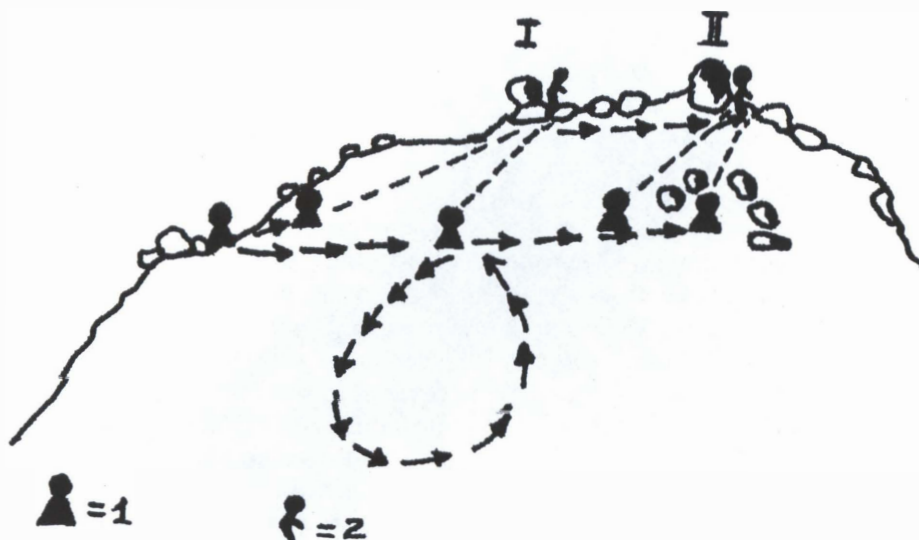


FIG. 3.—Positions of author (1) and Yeti (2) during encounter and photography.

leaving behind all unnecessary items, ascended the slope again, crawling toward my destination. On the first attempt, I miscalculated the distance, and stood up far from the object of photography. The sun came out again, and I took the first photo. The creature kept squatting and rising, and peering from behind the boulder. I could only see it when I stood up full height amidst the boulders. Hiding behind them, I resumed crawling, approaching the animal from the right-hand side (see Fig. 3).

A second photographic attempt was more successful, and I took three photos almost one after another. Then, keeping the animal in view, I began approaching it. The creature, alerted, bent down and looked towards me. I stopped, but it was too late; the animal moved behind the boulder, and presumably descended down the other side of the slope. When I reached the boulder, the animal had disappeared, and was not seen again.

Later that day, after my regular scientific work, I returned to the site of the encounter, and thoroughly examined the terrain within a radius of 2,600–3,300 feet (800–1,000 m). My findings were as follows:

1) Three large niche-shelters were found, which could have been used by the animal during the warmer time of the year (Fig. 4). The substrate was of a kind that did not show tracks, but it was disturbed, thus indicating that the niches had been used.

2) Feces resembling human coprolites, but darker, were found in two places, separated by a distance of about 330 feet (1,000 m); one sample (Fig. 5) was near the place of the encounter. The feces contained the remains of plant tissues, probably roots of plants of the genus *Saussurea* and others.

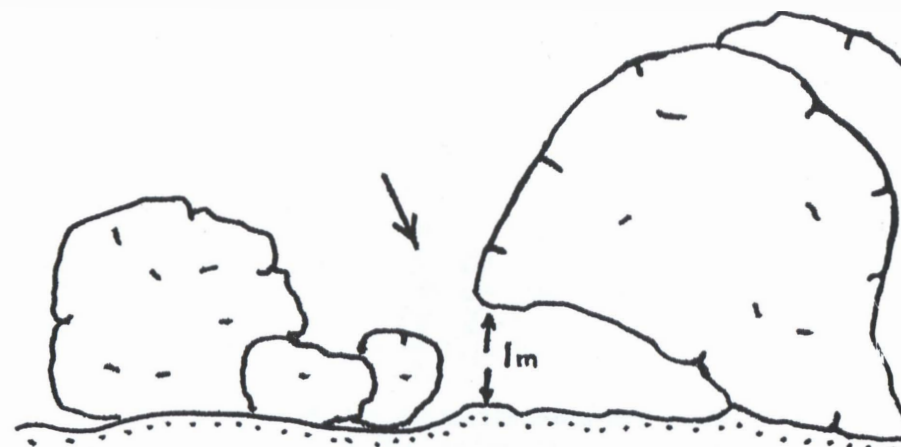


FIG. 4.—Illustration of niche, possibly used as a shelter by the Yeti.

3) On the slope of southern exposure, at an altitude of 16,100 feet (4,900 m), there were spots of dug out soil, 8 × 8 inches (20 × 20 cm), and up to 4 inches (10 cm) deep, with marks of bitten-off upper parts of roots and other underground plant organs (Fig. 6). It can be said with a high degree of probability that these were not the result of feeding activities by rodents or other known animals.



FIG. 5.—Presumed Yeti feces; 4 inches (10 cm) in length, and 1 inch (2.5 cm) in diameter.

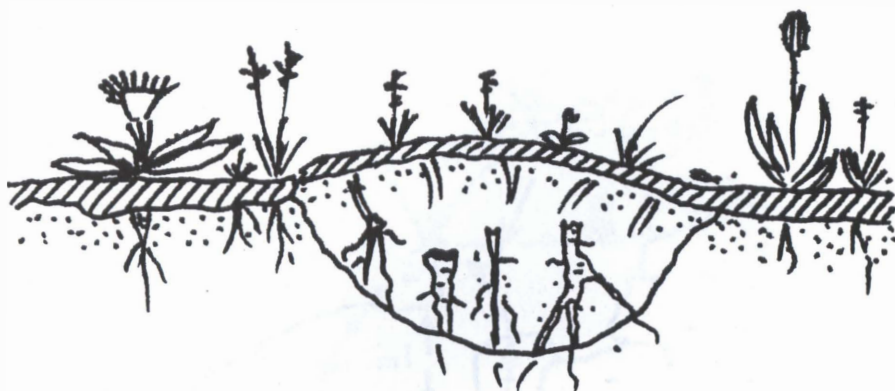


FIG. 6.—Illustration of dug-up roots that had presumably been bitten off by the Yeti.

4) Measurements of the boulder next to which the animal had been observed showed that its height in a slightly bent posture could not have exceeded 4 feet, 7 inches (1.4 m).

The questions I addressed that evening to the yak drivers were to no avail, as the Tibetans were reluctant to discuss any matters at all through the Chinese.

Two days later, the Expedition left that area, and after three days of travel and work in a neighbouring valley, returned to the base camp near Nyalam, on the Lhasa-Katmandu route. A month later, after difficult motor rides and hikes on foot in Tibet, we arrived in the city of Liangchou. Here, in a talk with the director of the local Institute of Glaciology and Geocryology, we learned that my Yeti encounter was not the only one in that area. Earlier, in the 1960's–1970's, during the first Chinese glaciological expeditions in the area of Mt. Xixiabangma, Chinese scientists are also said to have found evidence of the Yeti's presence at great altitudes, and reportedly had even attempted to catch one.

RESULTS

My observations of a human-like creature lasted, with interruptions, about one hour. Finally, presumably disturbed by my presence, the creature disappeared. I managed to take four photos with a Zenit-TTL camera, equipped with a regular lens, on ORWO color slide film (made in the former East Germany), from a distance of about 260 feet (80 m). The landscape and the feces were photographed on black and white film. The films were developed when I returned to Moscow. The results, alas, are insufficient as convincing evidence of the sighting: the silhouette of the creature in the photos is only observable through great magnification, about $\times 100$ –200, with the inevitable result of emulsion grains blurring the image.

The creature's "profile," based on my observations, is as follows:

- 1) The head is not big; it is cone-shaped, and is covered with dark-brown, apparently, short hair.
- 2) The neck, due to hair cover, was not visible, the head appearing to sit right on the shoulders.
- 3) The shoulders are sloping and narrow. The arms are long, and, when they are folded, the elbows jut out; when extended, the arms reach below the knees.
- 4) The legs are short, and a little flexed; on the knees, the hair is flattened or worn off.

The creature moves swiftly enough on two legs. It may occasionally take shelter in niches under big boulders, concealing the entrance with smaller boulders and stones. Shelters of longer duration may exist in more hospitable forested areas. During the summer and autumn period, it feeds, apparently, mainly on plants, including roots and other underground organs of alpine grasses. Its feces are of a dark, almost black color, containing plant fibers, and resembling human coprolites.

FUTURE PLANS

The author plans to continue his professional work devoted to the study of Himalayan vegetation dynamics. He has no direct plans connected with the Yeti problem. However, learning from experience, the author will be alert and better prepared for a possible new sighting of human-like animals occurring in the mountains.

THE PUMA IN NEW BRUNSWICK, CANADA: A PRELIMINARY SEARCH

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INTRODUCTION

Continued reports of pumas (*Puma concolor*) in northeastern North America led the American Ecological Research Institute (AERIE) to propose a winter-time investigation into the occurrence of the species in New Brunswick, Canada. Friends of the Eastern Panther (FOTEP), in cooperation with the Fundy Wilderness Coalition (FWC), allotted the minimum funding necessary to undertake this field effort. A team comprised of the two authors, plus Arizona puma biologist Harley G. Shaw, and two technicians, Craig Govan and Steve Gross, was organized, and fieldwork was conducted in March, 1992.

NARRATIVE DESCRIPTION

The five-person FOTEP field team arrived in New Brunswick on March 10, 1992. Fieldwork to locate evidence of pumas, known locally as panthers, began that evening and continued through March 26. Searches were made in the southern part of the province within an area of 500 miles² (1,295 km²) extending from the town of Belleisle Brook south to West Quaco, and northeast along the Bay of Fundy coast through Fundy National Park to Waterside (Fig. 1).

Additional reconnaissance was made over 3,100 miles² (8,029 km²), which, when coupled with the primary study area, takes in nearly the entire southern one-fifth of New Brunswick. Boundaries of this roughly rectangular area are delineated by St. George, Highway 770, and Fredericton Junction on the west, and Moncton and Riverside-Albert on the east (see Fig. 1). This larger area will henceforth be referred to as the study area.

Southern New Brunswick wildlands are primarily second or third growth boreal and northern mixed hardwood forest divided by extensive agricultural lands, large clearcuts, and monoculture plantations of jack pine and black spruce. Hunting camps, snowmobile trails, logging roads, settlements, villages, and towns regularly invade the forested landscape.

Several major rivers, including the Lepreau, Nerepis, St. John, Kennebecasis, Millstream, Big Salmon, and Point Wolfe, bisect the study area.

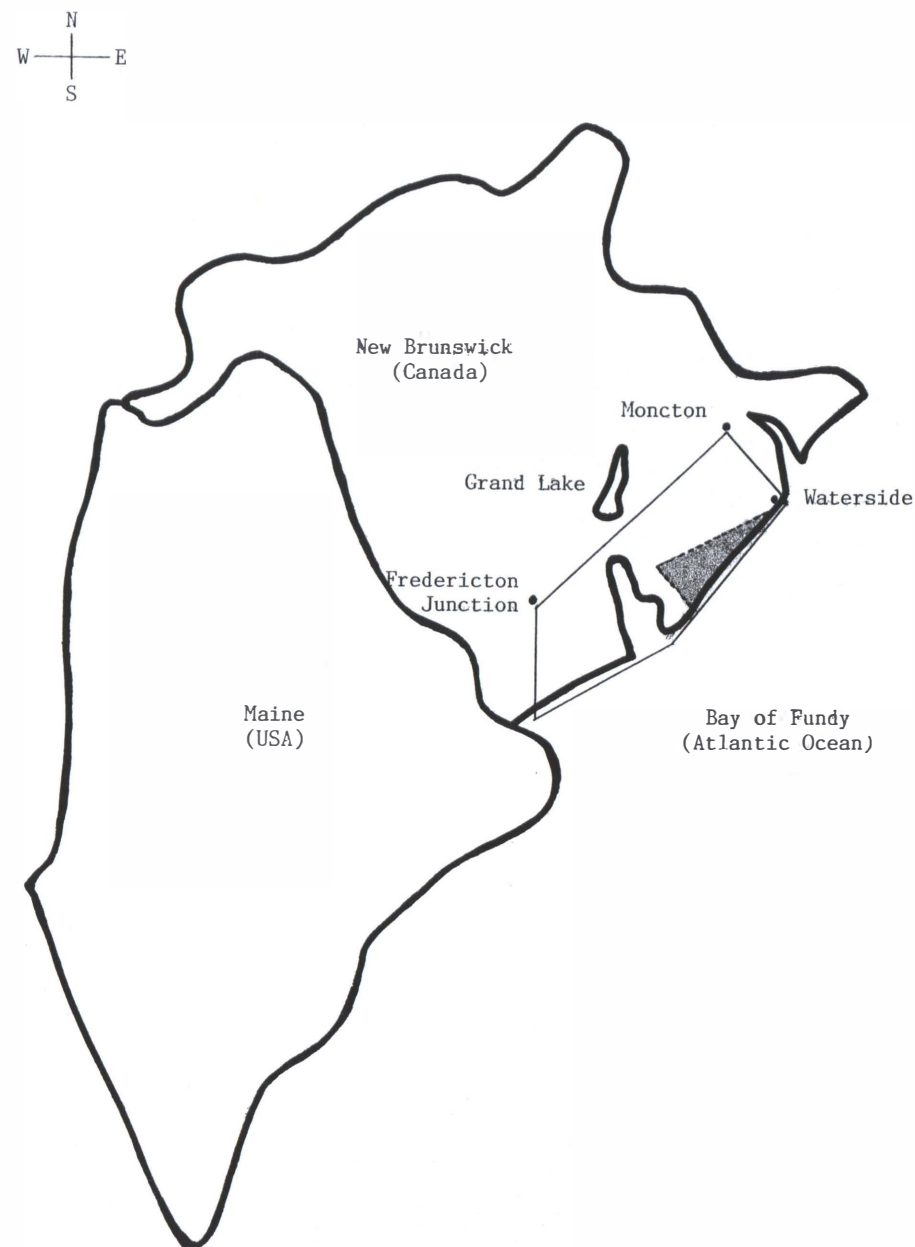


FIG. 1.—Southern New Brunswick study area of the 1992 FOTEP puma survey. Primary study area shaded. (1 cm = 25 miles/40 km.)

Their drainages are separated by a system of long, parallel ridges running to the Bay of Fundy. Elevations across the study area range from sea level to 1,300 feet (400 m).

Four-wheel drive vehicles and snowmobiles were the primary modes of travel during the survey. These vehicles were driven at slow speeds (5–8 mph/8–12 kph) along roads and trails while team members scanned for tracks, trails, scat, scrapes (scratches), or other evidence of puma presence. Searchers snowshoed or hiked whenever required by roughness or inaccessibility of terrain.

Typically, the team was divided into one to three crews, each consisting of one to three team members, with each crew afield from early morning to early evening in areas considered to encompass the best potential puma habitats. Our intent was to search known or suspected white-tailed deer (*Odocoileus virginianus*) wintering habitat.

Sign suspected to be that of pumas was evaluated by team members experienced with tracking and studying the species in the West prior to a final determination of its origin being made. When necessary, the surrounding area (0–3,300 feet/0–1,000 m) was thoroughly investigated. Reports of pumas or their sign made by persons unaffiliated with the study were investigated as soon as possible following team notification.

A light aircraft (Cessna 172) was used on one occasion to aid in the delineation of deer winter range, possible puma travel corridors according to terrain, and available cover within the primary study area. Data from this and all other field efforts were recorded on standardized forms retained by FOTEP.

RESULTS

Sixteen days were spent afield and 2,200 miles (3,542 km) traveled while reconnoitering habitat and searching for pumas and/or their sign across an area of 3,600 miles² (9,324 km²) in southern New Brunswick. Daily tracking conditions were as follows: six excellent (high probability of seeing tracks on at least 75 percent of route), two good (high probability on 50–74 percent of route), one fair (high probability on 25–49 percent of route), one poor (high probability on 0–24 percent of route), four variable, and two not recorded.

Three reports of pumas were investigated, two involving the sighting of single animals, and one involving the finding of tracks only. Evidence indicated that a coyote (*Canis latrans*), domestic dog (*Canis familiaris*), and domestic dog, respectively, accounted for the reports.

While the FOTEP field team found no evidence to indicate the presence of pumas in New Brunswick, the status of the animal necessarily remains a mystery. Pumas are solitary, secretive, highly mobile animals that, even where common, exist in populations of relatively low density. If, in fact, pumas do occur in the Northeast, it is likely their numbers are small. Doc-

umenting their presence and understanding even their most basic ecology will take a concerted effort far beyond the scope of a mere two and one-half week reconnaissance.

At the heart of the matter are questions of ecosystem suitability and integrity. The following quotes, taken from the team's daily data forms, exemplify this: "Just when you think you're getting into some good puma habitat, you come upon yet another house or village" (12 March). "Islands of halfway decent-looking habitat surrounded by either backwoods cabins, agricultural areas, or the Bay of Fundy" (12 March). "Lots of bland country; this area more populated than areas previously surveyed. Clearcuts" (22 March). "The prey base, cover, and topographical relief (Lake Utopia area) all suggest excellent puma habitat, although my sense of the area as a whole is that it is not big enough to support the animal" (23 March).

Much of the Acadian and boreal forest region we investigated could not support a viable, resident population of pumas. Essentially, the area offers little, and then only in ephemeral, seasonal fashion, of what pumas require. For example, the occasional winter-time concentrations of white-tailed deer that occur within the study area are insufficient to support pumas on a long-term basis, although transient and/or feral escaped or released captives (FERCs) could certainly drift through at any time of the year.

Despite these limitations, there is one area within the region that may offer promise as potential puma habitat. This area extends from the Fundy hills southwest of Riverside-Albert, including Fundy National Park and the rugged coastal wildlands to the Big Salmon River, northward through the Waterford area uplands to the Petticodiac deer winter range paralleling the TransCanada Highway Two (see Fig. 1). This zone may well represent a more diversified, more important continuum of habitat for any transient puma able to reach it. Again, however, it would be difficult for a population of pumas within this area to thrive.

Our conclusions are as follows:

- 1). No sign of pumas was observed during a two and one-half week reconnaissance of southern New Brunswick in March, 1992.
- 2). The status of the puma in New Brunswick remains uncertain.¹
- 3). Habitat within the southern New Brunswick study area is deteriorating rapidly within a context of industrial, residential, and recreational trends.
- 4). The study area probably could not independently support a resident population of pumas at this time.
- 5). In some instances, seemingly reliable observers mistake coyotes and dogs, and probably fishers (*Martes pennanti*), black bears (*Ursus ameri-*

¹ In March, 1993, the Minister of Natural Resources of New Brunswick announced that, based on new field evidence, pumas were officially recognized as occurring in that Canadian province. The new evidence originated from outside of the authors' study area—Editor.

canus), melanistic red foxes (*Vulpes vulpes*), and perhaps melanistic bobcats (*Lynx rufus*) as well, for pumas.

6). The legendary status the puma enjoys in the Northeast—perhaps due to its rarity and elusive nature—contributes greatly to what is apparently a sociological phenomenon of puma sightings.

Our recommendations are as follows:

1). Further active research is needed. Passive studies—the collection of puma reports, for instance—are in themselves no longer adequate. Priority at this time should be given to searching those areas of Nova Scotia, Quebec, and Ontario where ungulate winter range overlaps rugged and/or sparsely settled terrain.

2). In New Brunswick, a cooperative program (federal, provincial, and private) of systematic province-wide track counts should be initiated, not only to further investigate the status of puma, but, importantly, to monitor changes in habitat quality and shed light on the population trends of other sensitive wildlife as well.

FUTURE PLANS

A cooperative \$400,000 multi-year proposal for intensive puma research in New Brunswick has been submitted to the Canadian government.² Also, planning is underway for an eastern puma conference, co-sponsored by AERIE, FOTEP, the Eastern Puma Research Network, and the International Society of Cryptozoology.³

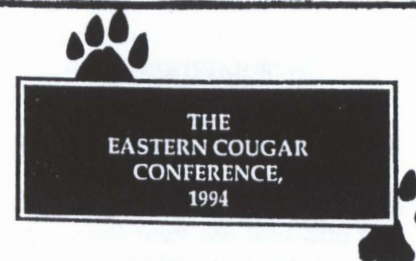
The authors wish to thank Steve Justason, treasurer of the Fundy Wilderness Coalition, for facilitating funding for this study. Local support was provided by Larry and Ida Adair, Sussex, New Brunswick, who opened their home and hunting camp to the FOTEP team. Rob Rainer served as liaison between the team and various agencies associated with this study. Housing, equipment, information, and field assistance were provided by Parks Canada and the New Brunswick Department of Natural Resources and Energy (NBDNRE).

Special thanks are extended to Chief Warden Pete Deering, George Sinclair, and Rob Walker, all of Fundy National Park. We would also like to thank NBDNRE employees Gerry Redmond, Rod Cumberland, Tom Pettigrew, John Bleness, Gary Moore, Ken Eagle, and Joey Kennedy, as well as members of the Eastern Cougar Recovery Team (ECRT), especially Bruce Johnson, team leader. Craig Govan, Valencia, Pennsylvania, and Steve Gross,

² Since this Field Report was submitted for publication, the proposal has been turned down—Editor.

³ Since this Field Report was submitted for publication, the Eastern Puma Conference was held at Gannon University, Erie, Pennsylvania, on June 3–5, 1994. The conference proceedings were published in 1996—Editor.

Burlington, Vermont, served as field assistants throughout the entire study. Their efforts are sincerely appreciated. Likewise, we are indebted to Malcolm Rossister, Alma, New Brunswick, who gave freely of his time and provided extensive knowledge of the region.



THE AMERICAN ECOLOGICAL RESEARCH INSTITUTE

Announces the Publication of the

PROCEEDINGS OF THE EASTERN COUGAR CONFERENCE, 1994

Jay W. Tischendorf and Steve Ropski, eds.



The Eastern Cougar Conference, held at Gannon University, in Erie, Pennsylvania, in June, 1994, was the first ever gathering devoted exclusively to the cougar (also known as puma and mountain lion) in the East. This historic conference was attended by people from 22 U.S. states and three other countries.

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SASQUATCH INVESTIGATIONS IN THE PACIFIC NORTHWEST, 1993

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INTRODUCTION

The 1993 investigations given in this report update my previous findings in this journal concerning attempts to find more evidence of the Sasquatch (Bigfoot), a reported large unknown primate (James A. Hewkin, 1992, *Sasquatch Investigations in the Pacific Northwest*, 1992, *Cryptozoology*, Vol. 11: 109–12).

Several non-productive excursions were conducted in the Cascade Mountains of Oregon, and a two-day trip to the Blue Mountains of eastern Oregon. Four interviews were transacted with people concerning possible sightings of Sasquatch.

NARRATIVE DESCRIPTION

The period of July 15–16, 1993, was spent with Jack Sullivan in the Blue Mountains, where we have consistently found possible sign of Sasquatch evidence in the form of torn up logs and stumps. We did succeed in locating four places where logs and stumps had been torn up, but no conclusive evidence was present in the form of track or nail marks to reach any determination of the animal involved.

Of particular interest was an examination of a pine stump located on the edge of a logged area. The stump, about 30 inches (76 cm) in diameter, had the bark pulled from it, with the pieces lying on the ground in a circular arrangement around the stump, with the exception of five pieces piled on top of the stump. A human could have done this, possibly gathering wood for a camp fire, but there was no evidence of a camp site in the immediate area. We considered the possibility that a Sasquatch could have placed some pieces on the stump while tearing off bark and examining it for food items, such as mice, insects, and reptiles. We once found slabs of fresh torn up wood placed on logs in the Cascades in 1975 (James A. Hewkin, 1986, *Investigating Sasquatch Evidence in the Pacific Northwest*; *Cryptozoology*, Vol. 5: 1986, 27–37).

On October 23, I visited a deer hunting area with Dave Davis and his family, who had camped and hunted in the area during the first and second weeks of October. The camp, consisting of eight people living in a tent, van, and motor-home, was set up on Squaw Creek, near Sister, Oregon, on the

eastern slope of the Cascade Mountains. They also kept two dogs, a poodle and a monstrous rotweiler.

The Davis family members are avid hunters, who look forward each season to hunt both deer and elk, but they were not prepared for the strange events that occurred on this occasion.

It appeared that something was hanging around the camp each night. Muffled sounds, possibly footsteps, could be heard. Mumbled noises were also heard, although nothing was seen and no tracks were noted. Both dogs were uneasy; one would bark, and the other would growl.

On October 4, Davis and his 11-year-old daughter, Angela, were preparing a lunch following a morning hunt. Davis was getting the Coleman stove ready to heat some water, when his daughter called him to look at something. He was reluctant at first, but then she told him to hurry. Thinking maybe she had seen a deer, he grabbed his rifle, but she merely pointed to a strange track on the ground. Further investigation revealed three human-like footprints with 5 toes on each; all three tracks were of a different size, one large, one medium, and one small.

The next day, they returned to the site with plaster Davis had bought, and attempted to make a cast of one of the footprints. While working with the plaster, Davis thought that he saw a dark-colored animal moving behind some ceanothus brush about 120 feet (36 m) away. Then his daughter saw, by a tree, a dark, hairy head with two black round eyes looking at them. The body could not be seen, but the head was bare in the forehead region, and sloped somewhat to a dome. Details of the nose and mouth area were not clearly seen. Davis leveled his rifle to get a clear view through its telescopic sight, but the animal was never in the same spot long enough to be seen clearly. His daughter was alarmed at first, because she thought it was a hunter and that her father should not be pointing his rifle in that direction.

In retrospect, they thought the experience lasted from 3 to 5 minutes, and probably less. Davis believed the creature must have stood about 5 feet (1.5 m) tall; Angela thought it was more like 4 feet (1.2 m) tall. They left in a hurry, and drove to camp very alarmed about the incident. Both witnesses agreed that the relatively short creature was very dark, with a reddish tinge. The footprint cast did not turn out to be worthwhile.

A few days later, Davis was hunting on a spur road winding up a steep slope where timber was quite dense. He found tracks spaced about four feet (1.2 m) apart across a cut bank in soft soil. The tracks led to a rotted log which had been recently torn up and was lying across the bank. There was no toe detail in the soft soil, but above the bank and back a few yards, under the timber, he noted a widespread toe print with no heel. He later returned with plaster and succeeded in getting a fairly good impression of the toes.

On October 23, when I investigated the area, there was a faintly noticeable footprint alongside the road at the site where Davis had seen the creature.

This print measured 11 inches (28 cm) long, 5 inches (13 cm) across the ball, and 3 inches (7.5 cm) across the heel. These measurements are slightly larger than my own bare footprint.

Much of this area is park-like, with open stands of yellow pine, and an understory of scattered bitter brush and ceanothus brush. There is a noticeable population of rodents, including chipmunks, golden-mantled ground squirrels, and chickarees. Davis stated that deer were scarce in 1993. He saw only five deer during the whole season. In 1992, in the same area, he had seen that many deer every day. Squaw Creek originates in the Three Sisters Wilderness, within the Willamette National Forest. The area has a history of Sasquatch reports.

An interesting unsolicited report was also related to me by Clayton Horn, of Pendleton, Oregon. He stated that his two brothers, now deceased, observed and followed huge human-like tracks while elk hunting on the north fork of the Umatilla River. He believed the year was either 1969 or 1970. He said his brothers were suspicious of fakery, and thought maybe someone on stilts or in a helicopter was responsible. They changed their minds when they followed the tracks up a long slope and into a dense stand of lodge pole pine. They then left the scene, rather frightened. Horn said he used to tease his brothers about those big footprints. This was before Bigfoot was talked about in the Blue Mountains area. This is another report that can be related to the Walla Walla situation. The Umatilla drainage is south of the Walla Walla drainage, and not many miles from the 1983 Blue Mountains activity (Grover S. Krantz, 1983, *Anatomy and Dermatoglyphics of Three Sasquatch Footprints*. *Cryptozoology*, Vol. 2: 53-81).

On October 3, 1993, I visited a site in the Western Cascades with Chris Johnson, of Stayton, Oregon, who, in 1988, reported seeing a family of three Sasquatches while bow-hunting for elk. The location was near Snow Mountain, not far from Stayton. There are extensive private timber holdings in this area, along with a mix of public National Forest timber at higher elevations. The topography is very rugged, consisting of steep-sided canyons and ridges. Vast stands of timber exist in various stages of reproductive growth, including a few patches of old growth fir.

Johnson was sitting beside a beaver pond, looking around, when he became aware of three Sasquatches across the pond, at a distance of over 300 feet (90 m). What appeared to be a male was sitting with its back against a large tree. What appeared to be a female was stretched out a few feet away, and, facing him, a juvenile was sitting against the belly of the mother. The two adults were looking at him, but the juvenile did not seem to be aware of him. Johnson left immediately. According to the witness, this site has changed significantly since 1988. The pond is no longer active with beaver, and has almost dried up and changed into a lush meadow; elk use it heavily, and bed down in it. The witness also showed me four rock pits a short distance

from the meadow. He had found these a year or so before. The largest pit was about 3 feet (91 cm) deep. The pits were old, and had caved-in considerably. It is not an extensive outcrop, and is surrounded by brush.

On December 15, Francis Williams and I interviewed an elk hunter who told of a strange experience during the 1993 elk bow-hunting season. The hunter, Spencer Farrell, shot a spike elk on September 29, just before dark. He was not successful in finding it, so his partner and he returned the next morning at first light, and found the elk about 75 yards (68 m) from where it had been shot. However, it was in an unusual position. The animal lay on its back, with its feet up in the air, with a tear in the lower abdomen running to the vent. The intestines had been pulled out and were dangling over a limb about five feet (1.5 m) above the carcass. The heart, liver, and paunch were still intact in the body cavity. Surprisingly, Spencer finished dressing out the carcass, claiming that the meat was good. Normally, an elk carcass will spoil and be unfit to eat if left undressed for a short time.

Francis, who is a retired government predator trapper, was of the opinion that a Sasquatch may have found the carcass, and then abandoned it because of the return of the hunters. The position of the carcass and the presence of the entrails dangling from a limb five feet (1.5 m) above is not the normal manner known predators handle an elk kill. This site is located in the Cascades, near Lebanon, Oregon, in the Crab Creek drainage. The area holds good populations of elk, deer, and rabbit.

RESULTS

There appears to be increasing evidence, though still scanty, that Sasquatch is both very illusive and widespread in its rugged habitat, but not abundant. Also, increasing evidence seems to show that it is an efficient predator and/or scavenger.

FUTURE PLANS

Future fieldwork will be conducted in attempts to uncover additional evidence of the presence and behavior of this supposed unknown, giant forest primate.

Book Reviews

Cryptozoology, 12, 1993–1996, 76–90
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Noah's Choice: True Stories of Extinction and Survival. By David Day.
Viking/Penguin, London, 1990. 169 pp. £ 7.99 (c.).

This is a book for cryptozoological parents. It would make a stimulating birthday present or stocking filler for children from eight to 18. Twenty years ago, a similar book initiated my own interest in cryptozoology.

David Day presents a problem. If an alien Noah had arrived on Earth during the Age of the Dinosaurs, which species would he have saved? What would have been his opinion of the tiny mouse-like rodents scurrying around the feet of the dinosaurs? Might he have thought of them as unfit species already on the brink of extinction because they were unable to compete with their larger neighbours? Would he have considered the rodents' extinction as inevitable, even morally right? Faced with such a choice, the alien Noah may have condemned the rodents, and all subsequent mammals, to death.

Our own species is now faced with similar choices. Day reviews our dismal record. The emphasis is firmly on human involvement in every recent animal extinction. He splits the book into two parts, each of nine chapters.

Under "Extinction" he relates the stories under the following headings: Dodos and Solitaires; Passenger Pigeon; Carolina Parakeet; Great Auk; Quagga and Blue Buck; Stellar's Sea Cow and Caribbean Monk Seal; Huia and Warrah; Ivory-billed Woodpecker and Dwarf Caribou; Stephen Islands Wren and Hawaiian Honeycreepers; Golden Lion and Bali Tiger; Thylacine and Mexican Silver Grizzly. The headings under "Survival" are: Takahe and Coelacanth; Golden Hamster and Przewalski's Wild Horse; Great Whales; Mountain Gorilla and Giant Panda; Hangul and Pere David's Deer; Whooping and Other Cranes; Egret and Sea Otter; American Bison and Wisent; Arabian Oryx and Peregrine Falcon.

From this list, it is clear that Day covers most of the infamous extinctions and the famous survival efforts. Pencil drawings by Mick Loates beautifully illustrate the main species in each chapter. Each history is related in clear, usually non-emotive language. Descriptions, dates, population numbers, and locations are given within about 1,500 words, occasionally through contemporary quotations. This reporting style lets the full horror of each extinction or near extinction shine through.

For the hardened, adult cryptozoologist, there are several histories worthy

of note. In ten years of professional biology, I'd never come across the warrah (the Falkland Islands wolf) or the hangul (a Kashmir subspecies of red deer). Nor had I heard of the dwarf caribou, yet its story should be engraved in the heart of every cryptozoologist.

Briefly, Dawson's dwarf caribou (*Rangifer tarandus dawsonii*) inhabited Canada's Queen Charlotte Islands. This small swamp-dweller seemed to be unknown even to the local Haida Indians. When fur trappers on the mainland spread rumours of the caribou's existence, G. M. Dawson heard them, and he alerted the scientific community. Physical proof was demanded. Then a reward was offered. Hunters produced skull fragments, which were not accepted as conclusive. Two expeditions found cryptozoological evidence; i.e., stories, sightings, tracks, dung, hair, and a horn. The rewards were increased. Hunters rushed in. On November 1, 1908, a herd of two bulls, a cow, and a calf fell to the guns.

The scientific debate was settled. It is now considered a subspecies, but no trace of Dawson's dwarf caribou has ever been found again. Neither hide nor hair.

Although it is dangerous to compare case histories, the dwarf caribou's extinction brings to mind the Sasquatch and the proposed "shoot to kill" method of obtaining conclusive evidence. It is difficult not to make comparisons when reading of the dwarf caribou, or Day's section on the ivory-billed woodpecker: "It has been estimated that the maximum number of Ivory-billed Woodpeckers which existed at any one time in America could only have been about three hundred." We can only hope that Sasquatch numbers in the thousands and not the hundreds.

Day provides a comprehensive index. It is understandable that there are no references or sources in a book for young people. However, it is a shame that Day does not provide suggestions for further reading for individual topics and the field in general. A list of relevant societies or information sources may have stimulated some youngsters into action. On the other hand, the lack of any sort of list at the end helps present this book as a good read rather than a textbook. If this stimulates a wider range of young people to read it, then all the better.

By clearly explaining the base stories, Day reminds all of his readers of the importance of our field of interest. Personally, he has restimulated my hope that Steller's sea cow or the great auk may still be hanging on somewhere.

This is a sobering, well-presented book to introduce young people to the facts, and to remind adults that extinction is forever.

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A Parrot Without a Name: The Search for the Last Unknown Birds on Earth.

By Don Stap. Alfred A. Knopf, New York, 1990. 240 pp. \$19.95. (c.)

Excluding Antarctica, South America is the least explored continent on Earth, and the Peruvian Amazon is one of the least explored regions of the South American tropics. It is a vast area, containing, among other things, the largest concentration of bird species in the world. All of Peru (including the montane Andean region, where in fact most new bird species discoveries are now being made) harbors no less than 1,700 bird species, over double the number found in the entire continent of North America. In fact, 18 percent of all the bird species in the world are to be found in this one country, which represents an area of less than 1 percent of the planet's land surface. It is there that Louisiana State University (LSU) ornithologist John O'Neill has been doing fieldwork since the early 1960's—and revolutionizing Neotropical ornithology in the process.

O'Neill was never satisfied with just routine academic life. He wanted to collect bird specimens himself, to see what they looked like in life—and to paint them. He was not content with merely examining museum specimens that others had collected years or decades before. He also had the notion that there were still undiscovered species to be found, a notion that ran counter to the proclamation by the great evolutionist Ernst ("Mr. Species") Mayr, who had written in 1943: "... It is safe to say that practically all the widespread species of birds of the world have been discovered, whether they be rare or common"—a statement he retracted in 1971. Thus, at 21, O'Neill went to Peru and discovered his first new species, and he hasn't stopped since. He has personally been involved in 12 bird species discoveries, more than any other living ornithologist.

In this book, author Don Stap narrates the story of the last LSU expedition to Peru, the 1987 Cordillera Divisor Expedition. It was O'Neill's 22nd expedition to that country in 25 years, and it resulted in the finding of a new species of parrot (his 12th discovery). The 15-member team included, besides Stap and others, Al Gentry, a world-renowned botanist from the Missouri Botanical Gardens, Tony Meyer, an M.D. and ornithologist, Angelo Capparella, an ornithologist now at Illinois State University, in Normal (and a new member of the Board of Directors of the International Society of Cryptozoology), as well as two Peruvian ornithology students and two American herpetologists.

The expedition departed by dugout canoes from the large jungle town of Pucallpa, on the Ucayali River, a major tributary of the Amazon. I remember Pucallpa—I spent 6 years in Peru in the 1960's—as muddy streets and simple amenities, as impoverished houses and vultures scavenging in the market. According to Stap, it is the same now, only bigger.

Following the river southeastwards, the expedition entered a tributary,

the Abujao River, which in turn led to the Shesha River. Following the Shesha northeastwards, the team eventually reached its destination: the Cordillera Divisor, a geomorphologically unusual mountain range in the midst of lowland tropical forest which almost certainly had never been visited by outsiders.

Stap's narrative is given, of course, from his own perspective, and as he describes the trials and frustrations that befell the team members—stolen equipment, an insufficient number of dugouts, low river water, and the usual dangers and maladies encountered in remote tropical areas—one is left wondering how the other participants would have described the adventure.

Interwoven with the expedition narrative is the story of South American ornithology, mainly in the persons of O'Neill and Ted Parker, a young but legendary figure who, although not on this expedition, repeatedly surfaces throughout the book. Parker became interested in birds early in life, and his dedication to South American birds became obsessive. In 1971, while still in high school, he established a new record for spotting the ca. 700 U.S. bird species in one year: 626. Later, when in Peru, he won the World Big Day record by spotting 331 bird species in a 24-hour-period. (Yes, most ornithologists are quite mad, and this is particularly so with those who work in South America.)

When he began working in Peru, Parker would spend months, sometimes years, doing continuous fieldwork. By the early 1990's, nobody knew as much as he concerning the status, distribution, and even the calls of the almost 3,000 birds of South America. The middle-aged and more conservative O'Neill and the young and iconoclastic Parker (whom I met in the 1970's when he was a student at the University of Arizona) are certainly the heroes of the story. Tragically, in August of 1993, Parker and Al Gentry were killed in a plane crash while surveying tropical habitat in Ecuador, and the world lost two premier tropical biologists.

Despite hardships, the 1987 Cordillera Divisor Expedition attained its main goals. It established two camps (which became frequented by jaguars), and collected almost 1,500 bird specimens representing 426 species, resulting in sixteen range extensions and one new species—the parrot without a name. In an epilogue, Stap relates how the expected Peruvian government collecting permit never did materialize, resulting in all the specimens save four having to be left in Lima's Museum of Natural History. However, Angelo Capparella recently informed me that the bureaucratic impasse has now been resolved, and that LSU has received half of the collected specimens—as originally planned.

Many parts of South America are still very wild and remote, remaining almost untouched since Humboldt, Wallace, and Bates. After all, it was only as recently as 1911 that Hiram Bingham discovered the gigantic Inca construction called Machu Picchu. This book brings the fact of that remoteness

to life. Parker himself is quoted in the book as stating that South America is "in the same condition that Bates and Darwin saw it. And it's that way right now."

Stap is a good writer, and this book is well written. Only a few errors jumped out at me: the Andes did not evolve about 10,000 years ago, as stated; they evolved gradually over the past several tens of million of years. The Cordillera Divisor certainly does not cover an area of 300,000 square miles; a rough estimate gives a more modest 10,000 square miles—still a large area of tropical forest, to be sure. The DC-3's that used to skim the tops of the Andes to take one down to the Amazon—and, as I well remember, when one had to insert a rubber tube in one's mouth to get oxygen!—were certainly not prop-jet jobs (only two prop-jet DC-3's were ever built, for private buyers). And, although not an error, I think it was less meaningful for Stap to point out that tropical rain forests cover less than 2 percent of the planet's surface than to have emphasized that they actually represent almost 7 percent of the planet's land area—although both statements are correct.

One can safely predict—unlike Mayr's 1943 prediction—that there are yet *more* undiscovered bird species in Peru, to say nothing of mammals and heaven knows what else. However, following the 1987 expedition, O'Neill put all LSU fieldwork on hold until there is less danger posed in remote areas by the Shinning Path terrorist movement—not to mention trigger-happy, government counter-insurgency troops.

As for the parrot without a name, it now has once. It was described by O'Neill in April, 1991, in *The Auk* (Vol. 108: 225–29), as *Nannopsittaca dachilleae*, in honor of Barbara D'Achille, a Peruvian journalist who championed the environment and was senselessly killed with others during a Shinning Path attack on her bus. It became the world's 333rd known parrot species.

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Monstrum! A Wizard's Tale. By Tony "Doc" Shiels. Fortean Tomes, London, 1990. 144 pp. £8.95 (p.).

When British stage magician "Doc" Shiels and some fellow psychics announced their project to bring Nessie to the surface for photographing, I was surely not alone among cryptozoologists in venting a mental groan. When

Shiels announced success and published two photos, I didn't much care for them.

When my friend Tim Dinsdale told me he gave Shiels credence, and distributed Shiels' affidavit to the authenticity of the photos, I looked for loopholes in the affidavit; could it be only *apparently* an *unequivocal* claim of truth-telling? And when I heard that Shiels was publishing this book, I looked forward to a better basis for deciding whether or not to believe him. (In the meantime, a friend with contacts in magicians' circles had told me that Shiels had confessed that the photos were trickery, and had shared with his fellow magicians the trick used; but later my friend told me that story had not been correct after all . . .).

Having read *Monstrum*, I doubt that Shiels is to be believed, and I remain unsure whether he wants or expects to be believed. In what is commonly called an age of science, he is a blatant romantic—in the strict sense of following impression, intuition, and poetry by contrast to attempts at objective observation, logical analysis, and intersubjective testing. Much of the book is stream-of-consciousness and free associating. The latter word-play he dubs "lexi-linking": ideas or events become (appear?) *actually* associated because of purely verbal associations between words of similar sound or usage, or happenstance of emerging into consciousness.

The term "lexi-linking" illustrates a common rhetorical device: by giving something a name, it is implied that there exists something to be named, and, moreover, that the thing has the characteristics denoted by the chosen name. Actually, there is no evidence given—or references cited—to support the idea that something of external reality underlies verbal coincidences. Rather, this is a throwback to the ages of magic, of incantation, of words and names that must not be pronounced lest harm ensue.

On the other hand, parts of the book read quite factually. Shiels clearly is familiar with the Nessie saga, and he gives an historically accurate account on a number of points. He also gives a two-page bibliography that includes not only Nessie material but also dragons, fiction, folklore, and philosophy. And there is a similarly eclectic mix of photographs, not only of Nessie and the Cornish sea monster Morgawr, but also carved stones, Nessie hunters, psychics, and naked witches.

Shiels' account of how he took the Nessie photos adds little if anything to the details already published by Dinsdale and others, and it certainly did not help me to decide what to believe. I still cannot see any loopholes in Shiels' affidavit. It does seem to say that he solemnly swears that the photos are genuine. But I recall an illuminating lecture by the psychologist Daryl Bem, pointing to the cultural difference between scientists and stage performers. The former are in the business of discovering objective truths, and any deluding of others is a sin; the latter, by contrast, are avowedly in the

business of producing illusions, and it is therefore not unethical for them to seek to delude. Perhaps Shields' stream-of-consciousness writing is intended to remind us not to take at face value anything he says. His straight-faced suggestion that Nessie is an "elephant squid" may be intended similarly, for the "elephant squid" has no warrant outside Shields' imagination.

I did find one clear instance of what one might call fibbing in the book. Shields reproduces (p. 24) "an illustration of two Nessie-like monsters by J. D. Batten, from *Celtic Fairy Tales* by J. Jacobs (1892)." Since I'm very anxious to find anything indubitably about Nessies, and indubitably put on paper before 1930, I took the trouble to get Jacobs' book. It is a collection of genuine fairy tales, and the illustration comes from a story ("The Sea Maiden") that has nothing whatever to do with Nessie-like monsters. The illustration actually shows two of the three heads of a most un-Nessie-like monster.

Which makes me wonder whether I can believe Shields—as I'd like to—when he tells us (p. 107) that the Amazing Randi (the arch-skeptic magician, psychic-baiter, and ex-CSICOPper) "is a Nessie fan."

Monstrum! belongs to the fanciful side of the Nessie literature, populated by such idiosyncratic people as Erik Beckjord, Roy Muir, and Frank Searle, and by such events as jammed cameras or films locked inaccessibly in bank vaults. Read it for that flavor, not to learn substantive things about the actual animals in Loch Ness.

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Dossier X: Les Hominidés Non Identifiés des Forêts d'Afrique [The X File: The Unidentified Hominids of the African Forests]. By Jacqueline Roumeguere-Eberhardt. Robert Laffont, Paris, 1990. 216 pages. 100ff (p.).

Each of Roumeguere-Eberhardt's books—not to mention her noteworthy films on the Masai rites made for French television—is part of a whole. If the reader is kind enough to share this assumption with me, I will be justified in providing a brief survey of the author's earlier works before tackling the heart of the matter.

Let us begin with *Pensée et Sociétés Africaines: Essais sur une Dialectique de Complémentarité Antagoniste chez les Bantu du Sud-Est* (African Thought and Society: Essays on a Dialectic of Antagonistic Complementarity Among

the Bantus of the Southeast, Publisud, Paris, 1986). These essays deal with an initiatic knowledge which is progressively unveiled as it becomes operational within our inner consciousness. They reveal the various aspects of a fundamental dialectic which originates from the masculine-feminine duality in a way such as these terms (feminine-masculine) appear as antagonistic and complementary; thus, the whole social and religious structure of the Bantus is based upon this duality.

In her first essay, part of which was published in English under the title "The Mythical Python Among the Vendas and the Fulanis: A Comparative Note" (*Archiv für Völkerkunde*, Vol. 12, Vienna, 1958), the author describes the *domba*, a traditional dance performed by the Vendas of Northern Transvaal. The dancers reenact a myth which may be summarized as follows: the python took a second wife, who was unaware of the real nature of her husband. During the day, she worked in the fields, and was prevented from going back to the village by the first wife. However, she found a pretext to return to the village; there, in the men's square, she saw the python, who fled in a fury and disappeared in the depths of Lake Fundudzi. There came a drought, and famine spread. Obviously, the python was claiming his younger wife, and he wished her to join him in the lake. So the royal maidens set to making the ritual beer, *mpambo*. When it was ready, the women took it to the lake as the men danced along and sang. The younger wife, bearing the beer pot, entered the lake and disappeared in it. Then the rain fell.

The *domba* is a fertility rite which renews the kingdom, secures the king's throne, and warrants plentiful harvests and fecund wives. The *domba*—literally, "the serpent which uncoils"—is God.

Originally, humankind and the whole of creation sat in the womb of the python who vomitted them. This explains why the initiates say that novices under training are "sitting in the Python's womb." The novices sit in the yard, surrounded by a fence which appears as the skin of the python. At the end of the *domba*, they will emerge from it as new creatures, as thorough persons. Python skins are placed around the fence, and a clay serpent hangs from the tree under which the novices dance. The novices thus learn that the fetus vomitted at birth originates from two serpents within the mother's womb: one of the serpents "belongs to the Gods," the other "belongs to men."

In another version (Fulani), the python forbade his twin brother to marry a maiden without breasts; i.e., who did not attend yet the school of puberty. In other words, a nubile girl. The brother disobeyed. The python said that the young wife should never look at him. She too disobeyed. Realizing that she was watching him while he was drinking, the python, in a flight of fury, destroyed her hut, and then fled to the river Niger. The cattle, the animals, and finally the whole of creation followed him. However, the python showed

some compassion: he told his brother to cut a twig, and to hit the animals with it. All the animals that the brother hit were saved. All the others disappeared into the water. This is why every prehistoric animal disappeared from the earth.

The mythical python is obviously linked with the notions of creation and procreation. A catastrophe occurs when the young maiden breaks a taboo; indeed, a nubile girl is not allowed to take part in the Dance of the Python, or *domba*.

Still, the ritual offerings (of beer by the Vendas, and of milk and butter by the Fulanis) are meant to overcome the rupture. The secrets involved in these offerings are taught in the initiatic schools, culminating in the *domba* precisely. These rituals restore a unity which had been broken. Through the *domba*, a dual—or dialectic—move is involved: on the one hand, the mythical reality becomes part of the social order; on the other hand, the social order falls into harmony with the mythical order.

The “masculine-feminine” antagonism underlies the religious and social structure of the Bantus. The author herself became an initiate through *venda* and *tsonga* teachings. She insists on the dynamics of such opposing principles, which are oriented towards creation and re-creation. Incidentally, as a *domba* initiate, the author (let us call her JRE) was given access to elements of knowledge which remain foreign to the lay person. For instance, JRE once asked about a musical instrument of the Lemba people called a *deza*. The matter, shape, colors, and various parts of the *deza* constitute symbols which reflect the mythology and social structure of the Lemba people. A Lemba priest revealed to JRE the “laws of the *deza*,” initiatic laws that one may “read” on the instrument, but he would not do it in the presence of his own brother, who, as a Christian, had received a “modern” education at school and was not a *domba* initiate. Playing the *deza* means to create, and the sound itself is the new-born child who is crying. The “laws of the *deza*” operate at three levels: individual, familial, and cosmic. JRE was offered a *deza* by the Lemba priest, who also authorized her to use at will the information he had provided her.

These brief samples from *Pensée et Société Africaines* are far from paying tribute to the whole book. However, the reader may trust the appreciation issued by the journal *American Anthropologist*, which hailed JRE’s collection of six essays as “an excellent contribution to the French school of Social Anthropology.” It also considered JRE’s work as following “the tradition of Mauss, Levi-Strauss and Griaule.”

This laudatory assessment would also apply to JRE’s previous book: *Le Signe du Début de Zimbabwe* (Zimbabwe’s Sign of the Origins, Publisud, Paris, 1982). This detailed and precise study is the result of 22 years of research on Bantu society. Although it deals with the qualitative rather than the quantitative, it is illustrated with numerous diagrams and charts—at

times quite complex—showing cosmic organization, social hierarchy, parental systems, and, most intriguing, their interrelationships.

In Part I, the role of the muputu (translated by JRE as “totem”) goes beyond that of the totem as studied precisely by Levi-Strauss (see *Le Totémisme Aujourd’hui*, Presses Universitaires de France, Paris, 1980). It then appears that the “totem” plays its vital part in everyday social life. It is real, not only intellectual; and, according to JRE, is bound to re-emerge under various forms, as the evolution of African countries already shows.

Part II concerns various myths as represented by the “bird of Zimbabwe,” phallic clay or canvas dolls, sacred rocks (Rock of Marriage, Rock of Incest, etc.), and Bushman cave paintings. JRE warns the reader that these are only bits and pieces from a vast initiatic knowledge which is to be acquired after many long years under the supervision of African masters.

Part III is about “real knowledge,” and supplies “operating models”: it explains how the dynastic and parental models, the cosmic and mythical models, operate and influence the basic social life of the Bantus. The book closes on similarities found within Masai society in Kenya. These two volumes published by Publisud should be considered sound scholarly studies. However, the layman may feel reluctant to do so when confronted by some of their complexities. In this case, he should acquire JRE’s *Les Maasai* (The Masai, Berger-Levrault, Paris, 1984), an album illustrated by the best color photographs I have ever seen on the subject. The text is clear and straightforward, and provides accurate data, as may be expected from an author who spent so many years among the Masai.

Quand le Python se Déroule (When the Python Uncoils, Robert Laffont, Paris, 1988), an autobiography by JRE, met great success with the general public. I find it disconcerting because of its odd chronology, stylistic weaknesses, and abundance of anecdotes. Perhaps this book should be approached as we would a sculpture of the Makonde people: typically, such sculptures display entwined bodies and limbs carved in hard ebony wood. According to the Makonde artists, those weird shapes are combined “so that you may *dis-understand*.” Nevertheless, on p. 10, in a prose poem (Prelude), reproduced below, JRE mentions X, an unidentified hominid of the Equatorial forests, bound either to live a parallel life or to disappear (my own literal translation):

“Mr X, our brother, our father, our prototype,
homo habilis or homo creatus,
Or then homo unknown, but homo yet
Whatever the specialists may say or think.”

Thus, in the opening pages of the book, the reader is confronted with the extraordinary Mr X “who lives next to us on parallel wavelengths.” Then Mr X vanishes until p. 229: there, a little more than one page is devoted to

the hominid. Fortunately, JRE's latest work, *Dossier X*, is more apt at satisfying our curiosity, although only partly.

The book consists mainly of 31 accounts selected from a corpus provided by 190 observers (including two Europeans, a Swede and an Englishman). Each observer is given a code number (the Englishman is known as observer 35), just like the 22 forests in which Mr. X has been encountered. JRE took this precaution so as to avoid the invasion of Kenyan forests by journalists and tourists. It would be tiresome to sum up the various accounts, especially as they are presented with an apparent lack of logical order. Oddly enough, one is likely to derive some pleasure from those repetitious reports, which basically tell the same stories. Besides, Bernard Heuvelmans' lengthy foreword (29 pages) serves as a useful guide, considering the intricacies of the text. Heuvelmans (shall we "code" him BH?) lists five types of hominids, summing up their main features:

X1: The tallest of them all, whose height ranges from 5 feet, 11 inches (1.8 m) to 8 feet, 2 inches (2.5 m). Known by the natives as "the one covered with moss," due to the short grey hair (reddish brown for the younger ones) on its body. Footprints are larger than those of a human. Hefty, peaceful, innocuous, and curious, X1 may seize a child or an adult, examine him/her and let him/her go unharmed. Armed with a club (a tree log or branch), X1 will knock down a buffalo, drink the blood from the jugular vein, eat the bowels, and even break off one whole foreleg and carry it on its shoulders. Among various hypotheses, BH suggests that X1 could be a specific African *Australopithecus*, adding that the discovery of a living specimen would be of the highest value to understanding the evolution of primates and the problem of our own origins.

X2: Very tall, but not hairy. Lightly colored skin (the Masai call him *naibor*, "the White"). Somewhat aggressive; camp-fires trigger its anger, causing it to rush at them, and scatter the embers with a stick while yelling. Unlike X1, it does not live in the forest, but in caves. Aside from its height, X2 resembles a Bushman similar to those who lived in East Africa thousands of years ago.

X3: Looks like a "negative proof" of X2. Dark skin, long hair (down to the thighs), and white due to its age: almost 100 years. Hunts, kills, and eats buffaloes in the same manner as X1. Speaks the Masai language. BH thinks X3 could be a solitary runaway warrior, a Masai delinquent.

X4: No taller than 4 feet, 3 inches (1.3 m). Dark skin, big head, short fuzzy hair, strong chest and neck, short bulky arms, and long sharp nails. Sometimes seen wearing a cape made from some animal hide. Feeds on roots and tubers that it unearths using a plain wooden stick. Also eats mushrooms, and is quite fond of honey. Does not hunt, but steals meat, eaten raw since it is ignorant of fire. In Tanzania, X4 is said to build small huts in the trees. Speaks a language connected with the Bantu linguistic group. Fears other men, and runs away from them. BH compares X4 to the

homuncules described in his book *Les Bêtes Humaines d'Afrique*, although X4 would better deserve to be classified as a "proto pygmy."

X5: It is not so much of an enigma, as, like X3, an individual case. X5 wears an animal hide, as well as laced sandals in the Roman fashion, carries a bag, a walking stick, a small bow, and poison arrows. JRE managed to purchase a bow, arrows, a quiver, and a bag from a native informer. Nobody either in Kenya or at the British Museum could identify X5's bow and arrows. Moreover, X5 uses a thick string made of elephant tendon, while the Ndoobos of the East African forests use zebra tendon. As a result, not a single Masai warrior was even able to bend the bow. However puzzling, X5, short, bald and elderly, is considered as yet another outcast by BH. Still, JRE believes that the capture of X5, who manages to survive incognito, would prove that unidentified hominids may easily hide in Kenya's forests.

BH, in spite of the affection and respect he bears to the author, remains critical towards JRE's text. He suggests, nonetheless, that the facts are presented in the African manner: pell-mell. "It is like an esoteric message from another world, very remote and at the same time secretly buried deep down in our inner selves." This book raises many questions concerning the real existence of Africa's hominids, the mythopoeitic or myth-making process—here applied to the archetypal Wildman—inherent in human nature, and the personality of JRE herself, whom BH, donning Trader Horn terminology, compares to the White Goddess.

In my own view, three tightly knit domains ought to be considered: the African background (which JRE described in her earlier works), the hominids (both under their "scientific" and archetypal aspects), and JRE with her particular approach. They should not be separated from one another. While observing how they influence each other, we might, in the long run, *dis-understand*, and at last learn something.

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Other Origins: The Search for the Giant Ape in Human Prehistory. By Russell Ciochon, John Olsen, and Jamie James. Bantam Books, New York, 1990. 262 pp., \$22.95 (c.).

We have here an adventure story about three Americans who excavated some Pleistocene fossils in Vietnam. Just as with *National Geographic* magazine and many popular books, the narrative is interspersed with a consid-

erable amount of scientific information written in layman's language. The science is human evolution in general, and the fossil ape *Gigantopithecus* in particular.

The pertinence of *Other Origins* to cryptozoology is the authors' description of Giganto and its possible relation to modern reports of oversized, hairy bipeds. Such a connection is indeed proposed, but it is the most bizarre idea I have ever run across. The description of how these men fared with their work, and with the Vietnamese, is well written and certainly makes fascinating reading. My further remarks here are all confined to the scientific aspects of this book.

The title of the book refers to origins of nonhuman higher primates, or to places of human origins outside of Africa—it is not clear which was meant. Most of the important aspects of human evolution are discussed, but in a confusing manner that jumps back and forth in both time and space. Well-informed anthropologists can find many interesting tidbits; the novice will be totally confused. Russell Ciochon is an accomplished paleoanthropologist at the University of Iowa; John Olsen is a respected archaeologist at the University of Arizona; Jamie James is a science writer who is credited with the actual writing of this book, according to the forward by Roger Lewin. For the sake of their professional reputations, I hope that the first two "authors" had little control over the final product. Most of the science is good, and the authors take a firm stand on some important issues where I also stand. It is mainly where matters of cryptozoology are involved that clear reasoning weakens, and sometimes fails altogether.

Gigantopithecus is known from the tooth-bearing parts of three jaws from China and one from India, and at least a thousand additional teeth from China and a few from Vietnam. The authors assign a date of 6.3 million years to the Indian jaw (opinions vary on this), while the latest teeth from the Far East may be only 400,000 years old. There are blank stretches in the fossil record that far exceed this time period. Much more factual data on Giganto is included, the most interesting probably being its diet. A whole chapter is devoted to an analysis of plant phytoliths that were still embedded in the teeth. These proved to be of the distasteful durian fruit rather than the expected bamboo. On the other hand, they noted that if an animal body is made twice as tall, retaining its same proportions, it will become almost four times as heavy; actually, simple geometry shows it will become eight times heavier.

That Giganto is extinct was specifically stated 43 times in this book, 20 of these in the chapter dealing just with that aspect. The "fact" of extinction was stressed so often that the reader may begin to wonder why the authors felt it was necessary to push this so hard. The absence of a given form from one point in the fossil record is never assurance that it did not live at that time and location. In three other places in the book one finds statements that the absence of a fossil is clear proof that the form in question did not

exist then and there (or anywhere near there). No paleontologist I know would be so rash as to assert, for example, that the absence of fossil elephantids in the United States and Canada dated at less than 10,000 years ago constitutes evidence that they became extinct in Central America at that time.

The authors absolutely rule out any possibility that there could be living Gigantos that might be used to explain the evidence for Sasquatch (Bigfoot), the Yeti, the Yeren, the Almas, etc. That there is *any* living species represented by these cryptids is specifically denied, though some of the evidence is discussed in considerable detail (surprising, given their stated opinion of it).

P. 225: "The evidence for the existence of Sasquatch, even more than for its Himalayan cousin, is entirely anecdotal." P. 226: "... for which we have no tangible evidence except for a few mysterious hairs and vaguely human-looking droppings." These statements imply that the thousands of eyewitnesses are *all* mistaken or lying; the authors completely ignore the many hundreds of sequences of clear tracks as though they didn't even exist. Further, in contrast with their above quotes, they go on to give a fair description of the 1967 Sasquatch film from northern California, and conclude (p. 226): "If the Patterson film is a hoax, it is an extremely elaborate one; and what is most surprising, given the immense amount of study devoted to it, is that it has not been definitively exposed." And they discuss the Minnesota Iceman that was studied by Bernard Heuvelmans and Ivan Sanderson, concluding (p. 233): "... yet it is nonetheless very hard to believe that two trained scientists, whatever their expectations might have been, could be completely deceived by a fake." But believe this they did, rejecting both the movie and the Iceman for reasons that were not stated. They even mention receiving letters from veterans of the Vietnam War stating they had been (p. 233) "... face to face with huge, hairy apes in the southeast Asian jungle ...". This was also ignored for no stated reason.

Rather than accept the evidence they are unable to discredit, the authors offer their own explanation—racial memory! Human ancestors of maybe 400,000 years ago, and more, were directly exposed to the Gigantos in East Asia. P. 233: "... the myth of the great ape is firmly, deeply, and inextricably embedded in the human consciousness." Thus, people today are presumably reporting experiences that happened to their ancestors at least 16,000 generations ago. The authors offer no suggestion as to how the "racial memory" of Sasquatch got into European ancestry; they specifically deny (as I do) that modern humans are descended from any single group of *Homo erectus*. And, one might ask, what happened to the "racial memories" of all the other animals and events that our distant ancestors must have experienced?

The authors attempt to bolster their idea with Panamanian Indian stories of elephantids that were extinct only in other parts of this continent 10,000 years ago. They also appeal to some native ground sloth tales in Patagonia

that are "less than 30 thousand years old," but which may be as little as only one or two thousand years old. Moving from accounts that are dubiously 10,000 years old to a single item that is 40 times farther back in time strikes me as an act of faith in total disregard for real data and scientific method. P. 229: "Yet the essence of the scientific method—that which makes a scientist different from a politician or a novelist—is that our explanations must, by definition, arise from data, not from wishful thinking. What *might* be is often enough for other professionals, but it is never enough for a scientist." No comment.

Getting a little closer to home is a lengthy quote from p. 228: "The case for relict populations of Giganto in North America are even more threadbare [than in Asia] Disregarding this overwhelming deterrent, a fervent Bigfoot believer named Grover Krantz, a professor at Washington State University, has gone so far as to assign to Sasquatch the name *Gigantopithecus blacki*. By doing so, Krantz has stepped outside the bounds of science; zoological names are always assigned to type specimens, palpable and verifiable examples of an animal, something which is lacking in the case of Sasquatch." The authors seem to have missed the simple fact that I did not name *Gigantopithecus blacki*; that was done over 50 years ago by Ralph von Koenigswald. I merely proposed that some modern footprints can be ascribed to that well-known fossil species. Perhaps they are unaware that paleontologists regularly assign tracks to known fossil species, such as dinosaurs, for example, and exhibit them as such in major museums.

Finally they note, p. 228: "Despite all the weight of scientific data and methodology that would tend to militate against them, well-established scientists around the world continue to join the ranks of those who subscribe to the existence of these anthropomorphic monsters." I would suggest that these scientists might be looking more carefully at the same evidence which has been so casually dismissed in this book. Evidently, the authors would rather that we scientists consider seriously that the human psyche has an image of Sasquatch that goes back 400,000 years to a now-extinct fossil ape. For this ape there are "type specimens, palpable and verifiable examples of an animal" whose extinction has been asserted, but hardly demonstrated.

At the same time, the authors expect us to accept, on faith, their notion of racial memory without any explanation whatsoever.

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Comments and Responses

This section permits readers to critique or comment on works previously published in Cryptozoology. The original authors and other readers are encouraged to respond to these critiques or comments. Readers are also encouraged to critique or comment on the works appearing in this issue. All comments are the responsibility of the authors only, and do not reflect any policies established by the Editor or the Editorial Board of Cryptozoology, or the Board of Directors of the Society.

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MORE ON MUSKOXEN

(Comment on Nikolai Spassov, 1991, The Musk Ox in Eurasia: Extinct at the Pleistocene-Holocene Boundary or Survivor to Historical Times? *Cryptozoology*, Vol. 10: 4–15)

Nikolai Spassov failed to cite another paper supporting his contention that the Mongolian ornamental plaques depict muskoxen. Julius Ehik (1932, Eine Interessante Angabe zum Vorkommen des Moschusochsen in Asien [An Interesting Report on the Occurrence of Muskoxen in Asia] *Zeitschrift für Säugetierkunde*, Vol. 7: 258–59) concluded that the artifacts displayed a very life-like depiction of a muskox. Ehik ridiculed a colleague who thought the depicted animals were probably yak by stating that (I paraphrase the German) maybe the artist *was* trying to portray yak, but if he was, he certainly used muskoxen as his models!

I concur with the conclusions of both Ehik and Spassov that the images depict muskoxen. While it is possible to point out aspects of the images that do not perfectly coincide with muskox morphology, they are far more accurate and realistic depictions than were the early illustrations of North American muskoxen in the scientific literature of Europe. While several writers have stated that the images do not, or could not, represent muskoxen, there is no agreement among such authors regarding what species is represented. In addition to the list of wild sheep, yak, takin, and takin-muskox composite noted by Spassov, I will add European bison, suggested by Nikolai Vereshchagin (1959, Ovtsebk na Severe Sibiri [The Muskox in Northern

Siberia], *Priroda*, Vol. 8: 105–06). Vereshchagin rejected the possibility that the plaques depicted muskoxen, an opinion apparently not based on a personal examination or any new evidence. Of course, I can only accept the validity of the plaques themselves based on the findings of other workers. Apparently, no one has questioned the age or origin of the artwork; the only debate is over the depicted species. That muskoxen can live in mountainous terrain with open wooded habitats is well established. Recent successful introductions and the establishment of herds in mountainous terrain of Alaska attest to that ability.

Even more pertinent are the small bands of introduced muskoxen in Norway and Sweden. These survive by occupying isolated patches of alpine habitat largely surrounded by forest and agricultural lands (Nils G. Lundh, 1991, *Myskoxarnas Hemområde och Biotopval* [Home Range and Habitat Selection of Muskoxen], *Fauna och Flora*, Vol. 86: 14–29). The question is not whether they can live in such habitats, but, rather, how did they survive for so long in this region of Asia, presumably exposed to hunting by substantial human populations, and in the face of competition from various alpine ungulates? The nearest other archaeological evidence for muskoxen is apparently from a late Paleolithic site in the Angara River valley, roughly 750 miles (1,200 km) northwest of Ulan Bator (Ermolova, 1978, cited in N. K. Vereshchagin and G. F. Baryshnikov, 1984, *Quaternary Mammalian Extinctions in Northern Eurasia*, In: Paul S. Martin and Richard G. Klein [eds.], *Quaternary Extinctions: A Prehistoric Revolution*, University of Arizona Press, Tucson).

It is now widely accepted that muskoxen survived in the Eurasian Arctic well into the Holocene. Nevertheless, the evidence for this continues to be extremely sparse, and all skeletal material of Holocene age seems to come from the Taimyr Peninsula. The contention of Vereshchagin (1959, above), that one such skull showed evidence of being pierced by a bullet does not seem to have been widely accepted in Russia. In fact, Vereshchagin makes no further mention of this in the 1984 paper cited above, presumably because the radiocarbon dates indicated the animal died long before the invention of firearms.

Spasov's remarks regarding muskoxen on the Taimyr Peninsula require some clarification and correction. Not only did Soviet biologists introduce to the Taimyr 10 muskoxen from Canada in 1974, but 20 were also taken there from Alaska in 1975 (Peter C. Lent, 1991, *Age and Reproductive Biology of Muskoxen Translocated to the USSR*, *Polar Record*, Vol. 27: 58–59). These combined stocks were held and bred in a fenced area on the Bikada River, near Lake Taimyr, until 1982. In 1979, Russian biologists undertook an initial experimental release of 13 animals. These stayed close to the enclosure for the first years (Sava M. Uspenski, 1984, *Muskoxen in the USSR, Some Results of and Perspectives on their Introduction*, *Biological Papers of the University of Alaska, Special Report No. 4*: 12–14).

If muskoxen from the introduction showed up on the northern coast of the Taimyr, the mechanism is not one of migration, as Spasov suggests, but rather of dispersal movements. The documented occurrence of muskoxen along this coast is a result of such dispersal. Indeed, Grigori Yakushkin (1989, *The Muskox Population of the Taymyr Peninsula*, *Second International Muskox Symposium*, Peter F. Flood, ed., National Research Council for Canada, A14–A15) reports that a lone male from the Bikada release was sighted at Cape Chelyushkin, in the same area where the two skulls were found in 1948. Such dispersal movements from introduction sites have been characteristic of most muskox releases. Initially, the individuals involved in these pioneering movements are almost always males. It is possible that a muskox escaped from the enclosed area prior to 1981, moved to the north coast, and died. Uspenski (1984) mentions two females that escaped, but one moved south and the other was found dead near the enclosure.

The above papers by Yakushkin and Uspenskii and others do not provide the specific dates of dispersal sightings. However, the remains of an individual that dispersed after the opening of the fences in 1981 would have been unmistakably fresh. Also, other parts of the skeleton probably would have been obvious, unless the skull had been transported from the place of death. Spasov does not report the sex and age at death of the individual whose skull was found by Muzchinkin in 1984. Such information could be helpful in unraveling the mystery.

Spasov was misled by incorrect information appearing in Vereshchagin and Baryshnikov (1984, above). It is unclear why these authors reported that both muskox introduction efforts in the U.S.S.R. had been unsuccessful, and that, therefore, "... this experiment showed that musk oxen could not inhabit large portions of the modern Eurasian arctic tundra" (Vereshchagin and Baryshnikov, 1984, above: 503). On the contrary, the success of the introductions on the Taimyr Peninsula, and the rapid increase and range expansion of the population (approximately 500 muskoxen by 1991) attests to the fact that much excellent habitat for muskoxen is present (G. D. Yakushkin and S. Barr, 1988, *The Introduced Muskoxen of Poluostrov Taymyr*, *Polar Record*, Vol. 24: 321–24; D. R. Klein, G. D. Yakushkin, and E. B. Posepelova, 1993, *Comparative Habitat Selection by Muskoxen Introduced to Northeastern Alaska and the Taimyr Peninsula, Russia*, *Rangifer*, Vol. 13: 21–26). Similarly, the introduction on Wrangel Island has been highly successful (D. N. Kovalev, 1990, *Sotsialno-demograficheskaya Struktura Ickucctbennoi Populiatsii Ovtsbykov [Ovibos moschatus] ostrova Vrangelia* [The Sociodemographic Structure of Introduced Muskox Populations on Wrangel Island], *Zoologicheskii Zhurnal*, Vol. 69: 120–31). The presence of so much suitable unoccupied habitat, as in Alaska and the Yukon Territory of Canada, supports the contention that the extinction of muskoxen across these regions was not due principally to habitat change.

Vereshchagin and Baryshnikov (1984, above) also concluded that the ex-

tion of the muskox in Asia proceeded from the south northward, with the last surviving population occurring in the northern Taimyr. Depiction of muskoxen on the Noin-Ula plaques raises questions about this generalization as well.

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ASIAN MUSKOX: MAIN PURPOSE ACHIEVED

(Response to Lent)

Lent presents two additional opinions concerning the animals depicted on the Noin-Ula (Hun) plaques from Mongolia that were not included in my paper, those of Nikolai Vereshchagin (1959, *Ovtsebk na Severe Sibiri* [The Muskox in Northern Siberia], *Priroda*, Vol. 8: 105–06) and Julius Ehik (1932, Eine Interessante Angabe zum Vorkommen des Moschusochsen in Asien [An Interesting Report on the Occurrence of Muskoxen in Asia], *Zeitschrift für Saugetierkunde*, Vol. 7: 258–59).

Vereshchagin indeed considered these 1st century A.D. plaque depictions to be of *zubr*; that is, a late Quaternary bison. (In this case, the Russian word *zubr* should not be interpreted in the narrow sense as meaning the modern European bison, *Bison bonasus*.) I should add, however, that, in one of our personal conversations in 1990, Vereshchagin did not reject my proposition that the depicted animals were muskoxen.

It is unfortunate that I was not aware of the 1932 paper by the renowned zoologist Julius Ehik. On the other hand, it is now gratifying for me to realize that, almost 60 years after the publication of his paper, I arrived at the same conclusion, and that my efforts to revive and confirm this rejected and practically forgotten—but very interesting—idea have been fruitful.

Lent also presents some very useful supplemental information. It is indeed not easy to imagine how an animal such as the muskox was able to survive in Asia until the beginning of the contemporary era (A.D.). It is also even more surprising, incidentally, that, in a much more densely populated region of Southeast Asia, there was discovered in the 1930's an unknown species of wild ox, the kouprey, and, just recently, another unknown species and genus (!) of bovid, and probably also an unknown species of muntjak deer.

It is true that, in northern Mongolia and adjacent hilly country, the Asian muskox would not have found the most appropriate habitat. What we are discussing, however, is the survival of several localized and isolated micro-

populations. That is why they did not survive to the present time. They probably did not enjoy optimal conditions, and their low numbers were then depleted by humans. It should also be emphasized that, in these regions of Asia, there are, and always have been, vast areas practically devoid of any continual human population.

In their conclusion that muskoxen could not now inhabit large regions of the modern Eurasian tundra, what Vereshchagin and Baryshnikov (1984, Quaternary Mammalian Extinctions in Northern Eurasia, *In*: Paul S. Martin and Richard G. Klein [eds.], *Quaternary Extinctions: A Prehistoric Revolution*, University of Arizona Press, Tucson) apparently had in mind was that the present tundra does not have the optimal bioproductivity of the Pleistocene tundra-steppe, and thus would not be able to support the former populations of muskoxen.

Unfortunately, I do not have information on the age and sex of the skull found by Muzchinkin. This information could probably be obtained from him. However, even if we assumed that some individuals of the introduced muskox populations did reach the northern coast of Taimyr in their natural aspiration for movement—in fact, the dispersal movements mentioned by Lent are just one form of aperiodical migrations—it is not likely that Muzchinkin, a zoologist, would have been unable to distinguish a skull that had been deposited for only several years from one that had been exposed for many decades or even centuries.

In the final analysis, I think that the main purpose of my paper concerning probable Asian muskox depictions from Hun burial mounds in Mongolia has been achieved. This topic has aroused the interest of numerous zoologists and paleontologists, and the consensus of opinion seems to be that the Asian muskox survived until almost the present time.

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THE STATUS OF *CRYPTOPHIDION ANNAMENSE*

(Comment on Van Wallach and Gwilym S. Jones, 1992, *Cryptophidion annamense*, a New Genus and Species of Cryptozoic Snake from Vietnam [Reptilia: Serpentes], *Cryptozoology*, Vol. 11: 1–37)

The description of the genus *Cryptophidion* by Wallach and Jones is based on the examination and interpretation of three poorly-focused photographic

color slides of a snake: two lateral views of the head, and a ventral view of the whole body.

The photographic slides had been taken during the Vietnam War by members of the U.S. Naval Medical Research Unit No. 2, who had been collecting specimens which they intended to donate to the Smithsonian Institution's National Museum of Natural History. As Wallach and Jones were not able to locate the specimen photographed, the type of the new taxon is presumed "to be lost." As this is undoubtedly a burrowing kind of snake, Wallach and Jones report having compared it with other kinds of burrowing snakes in order to identify the specific characters by which it differs from those other snakes.

We will now demonstrate that a) the specimen does not represent a new species, and b) the approach by Wallach and Jones is not scientific. It is our impression that possible candidate species known to occur in the region were not examined in order to evaluate the resemblance of the photographs to such species. Instead, Wallach and Jones make some rather questionable deductions from the photos in an attempt to describe a new cryptozoological taxon.

We have tackled the problem following the first approach, concluding that *Xenopeltis unicolor* is the (well-known!) species corresponding to the photographic data. In spite of the nice tables produced by Wallach and Jones, they did not even think of suggesting a close relationship with *Xenopeltis*. In order to prove the inaccuracy of the data in these tables, we checked both the literature and specimens in museum collections: we used the complete series of *Xenopeltis unicolor* specimens held at the Royal Institute of Natural Sciences of Belgium, Brussels (6 specimens: IRSNB 29, 29b, 1177, 3144, 12387, and 14570) and 19 specimens from the National Museum of Natural History, Paris (MNHN 626, 6299a & b, 7177a & b, 1884.70, 1894.106, 1896.630, 1920.203, 1962.276, 1970.414-417, 1973.138, 1974.1254, 1974.1443, and 1975.113-114).

When surveying the burrowing snakes living in Indochina, using the literature cited by Wallach and Jones, our attention was immediately drawn to the striking resemblance of the snake photographed with the genus *Xenopeltis* Reinhardt, 1826, which has a distribution spreading from Indochina to the Indo-Australian archipelago, but not including New Guinea and Australia (J. Deuve, 1970, *Serpents du Laos. Mémoires O.R.S.T.O.M.*, Vol. 39: 1-251; K. R. G. Welch, 1988, *Snakes of the Orient: A Checklist*. R. E. Krieger, Malabar, Florida).

We checked the differences presented by Wallach and Jones in their Table 2 (p. 18), which compares the two genera. Among the 26 characters used as the basis for the comparison, they found eight characters different among both genera. Hoping to ring a bell, we will discuss all eight of them.

Character One (ventral proportion): This is the ratio of the width between the ventral scales and the body. This ratio is indicated in Table 2 as being between 0.5 and 1 for *Cryptophidion*, and less than 0.5 for *Xenopeltis*. In the 23 IRSNB and MNHN *Xenopeltis* specimens we examined for this comparison, that ratio, measured at the hundredth ventral, varies between 0.56 and 0.78 for the maximal width of the ventral, and between 0.40 and 0.67 for the minimal width of the ventral.

Character Two (anal scale): This has been described as undivided in *Cryptophidion*, and divided in *Xenopeltis*. In all (25) IRSNB and MNHN *Xenopeltis* specimens examined, the last ventral scale is very big, larger than the preceding ventrals, and it has a triangular form, while the divided anal scale is very small. This feature is well illustrated in Georges Jan and Ferdinand Sordelli (1866, *Iconographie Générale des Ophidiens. Tome Premier [livrais 1 à 17]*, J.B. Baillière, Paris, 1860-66), and B. C. Mahendra (1938, *The Lepidosis of Xenopeltis unicolor* Reinw. *Current Science*, Vol. 6[11]: 559-60.) Apparently, this last ventral scale has been mistaken by Wallach and Jones for an undivided anal. Consequently, the total number of ventrals for the specimen must be 173 rather than 172.

Character Three (infralabials): Four infralabials supposedly contact the pregenials (sublinguals), whereas only three such infralabials do so in the case of *Xenopeltis*. Due to the poor quality of the photos, it is not easy to evaluate the situation in *Cryptophidion*. We, unlike Wallach and Jones, can only distinguish up to three infralabials in contact with the sublingual in the *Cryptophidion* photo. Moreover, all clearly visible scale outlines on the lower side of the head correspond perfectly with all the *Xenopeltis* specimens we examined. We find no reason to believe that four infralabials are in contact with the first sublingual.

Character Four (temporolabial scale): This scale, according to Wallach and Jones, is absent in *Xenopeltis*. It is, in fact, the anterior lower temporal separated from the parietal by the superior postocular. This is exactly the situation found in all of the 25 *Xenopeltis* specimens we examined. Moreover, it can be found in all illustrations of the head of *Xenopeltis* in the literature (Fig. 85, p. 276 in George A. Boulenger, 1890, *The Fauna of British India, Including Ceylon and Burma. Reptilia and Batrachia*. Taylor & Francis, London; plate V, p. 549, in Clifford H. Pope, 1935, *The Reptiles of China. Natural History of Asia, Vol. X*. The American Museum of Natural History, New York; Fig. 1, p. 559 in Mahendra, 1938. above; plate V in Hubert Saint-Girons, 1972, *Les Serpentes du Cambodge. Mémoires. Muséum National d'Histoire Naturelle, Zoologie, Ser. 2, Vol. 74: 1-170*).

Character Five (rostral profile): This is said to be pointed in *Cryptophidion*, and blunt in *Xenopeltis*. Within all the 25 IRSNB and MNHN *Xenopeltis* specimens examined, we found it very difficult to make a distinction between

a pointed and a blunt rostral for the genus; some specimens present a rather blunt shape of the rostral scale, while others can be considered to be pointed, at least at the same level as in *Cryptophidion*.

Character Six (vertical eye proportion): This character has been indicated to be 0.4 in *Cryptophidion*, and somewhere between 0.18 and 0.33 in *Xenopeltis*. The question here is to determine if (and how) the measurements of the snake's head in the photograph were corrected for the easily detectable inclined position of the head. Taking the head depth at eye level (taken over the lower jaw), we found the following values for *Cryptophidion* (both minimal and maximal interpretations are given): 0.31–0.33 (0.25–0.27 in the reconstructed outline) for Fig. 1, and 0.30–0.34 (0.34–0.38 in the reconstructed outline) for Fig. 2. In the case of our series (21 *Xenopeltis* specimens measured), the same ratio between the vertical eye diameter (measured at the left and at the right) and the total height of the head (with closed mouth) at eye level varied between 0.19 and 0.41. When the head depth is taken at eye level (vertically from mouth opening to top of head, i.e., without the lower jaw), we found the following values for *Cryptophidion*: 0.36–0.38 (0.30–0.33 in the reconstructed outline) for Fig. 1, and 0.39–0.41 (0.41–0.45 in the reconstructed outline) for Fig. 2. It is clear that these values all relate to the same animal. The use of this character is quite problematical; normally, to express the size of the eye, the diameter relative to the distance from the eye to the end of the snout is used (e.g., Hymen Marx and George R. Rabb, 1972, *Phyletic Analysis of Fifty Characters of Advanced Snakes. Fieldiana, Zoology*, Vol. 63: 1–321). Additionally, the measurement used as a reference (i.e., height of the head) is, in this case, on a preserved specimen, not easy to take, and not very reliable: in snakes, the upper-jaw is rather mobile, and the head can really be flattened while it broadens. This may be the case in the photographed specimen: the supralabials are inclined and appear less high compared with the comparative series.

Character Seven (horizontal eye proportion): This refers to the ratio between the length of the snout and the horizontal diameter of the eye (i.e., the inverted form of the standard relative eye width). This ratio is said to be 2.5 for *Cryptophidion*, and 3.3 to 6.4 for *Xenopeltis*. It varies between 2.95 (smallest specimen) and 5.39 in the 21 *Xenopeltis* specimens we measured. The ratios we find when measuring on the photos (minimal and maximal estimates) of *Cryptophidion* are 2.64–2.98 for Fig. 1, 3.54–3.86 for Fig. 2, and for the reconstructed outlines 2.30–2.55 and 3.40–3.84 respectively. Apparently, the ratio of 2.5 given by Wallach and Jones was based on the drawing in Fig. 2, which does not accurately reflect the information given in the photograph.

Character Eight (ventral coloration): Ventral color is said to be light in *Cryptophidion*, and dark in *Xenopeltis*. However, George A. Boulenger, (1893,

Catalogue of the Snakes in the British Museum [Natural History], Vol. I. British Museum [Natural History], London) stated clearly that the coloration of the body in *Xenopeltis unicolor* is "white beneath." Of the 24 specimens of *Xenopeltis unicolor* we examined, some have a completely white ventral side, but all the others have a beige-colored underside, slightly darkening towards the tail, with the tail itself having a brown underside. One specimen (MNHN 1884.70), sloughing, appears completely black.

We must conclude that none of the eight supposed differentiating characters provided by Wallach and Jones survives close analysis: the 26 characters enumerated in their Table 2 are therefore actually 26 characters that are common to both genera.

Moreover, still other similarities are mentioned in the text itself, thus reinforcing the thesis of the synonymy which we will be proposing. Wallach and Jones, for instance, recognize (p. 12) that the pre-ocular scale of *Xenopeltis* is remarkably similar to the "pre-orbital" of *Cryptophidion*. Furthermore, the primitive pattern of the supralabials (3:2:3) in *Xenopeltis* is said (pp. 8, 13) to have exactly the same appearance in *Cryptophidion*. Wallach and Jones assume that the pupil of *Cryptophidion*, while not visible on the photograph, is round, "upon the supposed cryptozoic habits of the snake" (p. 9, 28). One of us (OP) recently collected a *Xenopeltis unicolor* in Chiang Mai, Thailand (IRSNB 14570). When the specimen was still living, the pupil was not visible, and the eye was completely black, like in *Cryptophidion*. The pupil appeared white and contrasted only after several hours in formalin. Wallach and Jones suppose the number of longitudinal scale rows to be about 15; all the specimens of *X. unicolor* we examined had 15 scale rows, in accordance with the literature. Other characters we found in common are the formula of the temporals (1 + 2 + 3), and the undivided first subcaudal, while all following subcaudals are divided.

From a zoological point of view, the description of *Cryptophidion anamense* falls within the rules of the Code (International Commission on Zoological Nomenclature, 1985, *International Code of Zoological Nomenclature, Adopted by the XX General Assembly of the International Union of Biological Sciences*. 3rd ed. International Trust for Zoological Nomenclature, London), as the authors correctly explain in their paper. We also have to keep in mind, however, that the type is actually the illustrated specimen, and not the illustration itself, as specified in Article 72(c)(v), of the Code. Furthermore, the notion of iconotypes as defined by Donald L. Frizzell (1933, *Terminology of Types. American Midland Naturalist*, Vol. 14[6]: 637–68), and used by Wallach and Jones to designate their "type slides," is not mentioned in the Code.

Based on the same material examined by Wallach and Jones, although not seeing the type, we conclude that neither the genus *Cryptophidion* nor

the species *Cryptophidion annamense* Wallach and Jones, 1992 are valid taxa, and they should be placed in the synonymy of *Xenopeltis unicolor* Reinwardt, 1826.

The description of *Cryptophidion* is a good example of certain weaknesses in the Code. Other examples are *Nessiteras rhombopteryx* Scott and Rines, 1975, *Marsupilami franquini* Quintart, 1989, and *Cadborosaurus willsi* Bousfield and LeBlond, 1995 (see Olivier Pauwels and Frédéric Chérot, in press, *Cryptoherpétologie et Nomenclature: Le Problème et sa Solution*, *Bulletin de la Société Herpétologique de France*).

If cryptozoology, while handling the delicate situation of incomplete or ill-defined data, does not take its scientific impact seriously, trying to eliminate falsification and purposeful or clumsy misinterpretation, then maybe the Code's nomenclatural rules should be changed.

The senior author wishes to thank Georges Lenglet and Louis Walschaerts at the Royal Institute of Natural Sciences of Belgium, in Brussels, for their cooperation. Alain Dubois and Ivan Ineich of the National Museum of Natural History, in Paris, and Frédéric Chérot of the Free University of Brussels, made constructive comments on the manuscript. The permit for collecting the Thai *Xenopeltis* and other reptiles was obtained with the highly appreciated help of Wiroj Saengbangka of the Office of Agricultural Affairs, Royal Thai Embassy, Brussels.

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Response by the Editor

Concerning possible "falsification and purposeful or clumsy misinterpretation," all Articles and Research Reports published in *Cryptozoology* are first reviewed by two referees, who provide the Editor with recommendations

concerning publication or non-publication, as well as numerous specific editorial criticisms and/or suggestions. Seventy scholars have served as referees since 1982. In some instances, three referees are consulted. This was the case with the Wallach and Jones paper, as it was to be the first taxon scientifically described in *Cryptozoology*, was based on photographs only, and thus required particular evaluation. The three referees, one of whom serves on the journal's Editorial Board, are highly competent herpetologists, and there is no question of improper review of the Wallach and Jones paper prior to publication.

The journal's manuscript rejection rate since 1982 has been about 25 percent. (Even a paper by the Editor himself has been rejected twice based on referee recommendations, and awaits further revision.) In addition, *Cryptozoology* provides a unique forum, its *Comments and Responses* section, where, in the spirit of free scientific inquiry, criticism, and discussion, any individual may address the possible faults of previously published works—often to the annoyance of the original authors. This is what allows the above criticisms by Pauwels and Meirte themselves. Perhaps more than any other aspect, this self-examining process is what elevates the journal *Cryptozoology* to a standard of scientific scholarship.

The Editor.

CRYPTOPHIDION IS NOT XENOPELTIS

(Response to Pauwels and Meirte)

The sunbeam snake, *Xenopeltis unicolor*, is a rare but extremely well-known species of eastern Asia. It is a stout-bodied, nearly cylindrical form, tapering abruptly to the small head without a hint of neck. The tail is thick. Broad ventral plates extend to the mental (chin) region, infuriating herpetologists by violating the standard method of counting ventrals (in normal colubrid snakes they correspond to the number of vertebrae), *Xenopeltis* has a light belly, and small ones have a prominent light collar or neck ring. *Xenopeltis* looks like what it is: a burrowing boa.

As one of the original three editorial reviewers of the paper by Wallach and Jones, I agonized over *Cryptophidion*. I tried hard to see in the photos some known snake. I liked the colubrid genus *Trachischium* best, but *annamense* fit no described species of that, and Wallach and Jones provided major character distinctions. With its neck, relatively slim tail, near-flat belly, and normal anterior ventrals, *Cryptophidion* certainly looks like a colubrid snake. Nobody familiar with *Xenopeltis* could, in my opinion, seriously entertain the notion that the photographs are of that peculiar snake.

Cryptophidion is peculiar too, but in distinctly other ways. I conclude it is a valid taxon.

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CRYPTOPHIDION IS A VALID TAXON

(Response to Pauwels and Meitre)

In challenging the validity of *Cryptophidion*, Pauwels and Meitre indicate a lack of understanding of basic ophidian anatomy and physiognomy. *Cryptophidion annamense* is strikingly distinct from the booid *Xenopeltis unicolor* in morphological appearance, proportions, and gestalt. Specialists familiar with the higher taxa of snakes can recognize and discern them.

For example, scolecophidians (Anomalepididae, Typhlopidae, Leptotyphlopidae), uropeltids (Uropeltidae), booids (Xenopheltidae, Loxocemidae, Pythonidae, Boidae), viperids (Viperinae, Crotalinae), and hydrophiids (Hydrophiinae) are each recognizable based on features of head and body shape, coloration, scutellation, and proportions. There are exceptions, and it is sometimes difficult, even with a specimen in hand, to correctly determine the generic or familial relationships of certain snakes. However, a competent ophiologist can discriminate between most snakes of different families at a glance. *Cryptophidion* vs. *Xenopeltis* is a case in point. The description of *Cryptophidion* was not a spur-of-the moment decision. We studied, contemplated, and discussed the slides, and consulted with colleagues, for more than three years before preparing a manuscript for publication. We did so because we felt it was our responsibility to the scientific community to report the existence of this unusual new taxon.

During the past 20 years, the senior author (VW) has examined and dissected representatives of every family, subfamily, and tribe of snakes, including more than 95 percent (440 of approximately 460) of the world's genera, and more than 60 percent (1,645 of approximately 2,700) of the known species. Of all of the genera listed in Tables 2-5 of our paper, only *Emmochliophis* has not been personally examined by VW; *Pararhadinaea* and *Tetralepis* have been examined since our paper was published. In addition, for more than 30 years, VW has had experience with living specimens of various taxa from throughout the world. Most of the data incorporated

into Tables 2-5 of our paper came from personal observations and measurements; scale count ranges were taken from the literature. The tables were not intended to be an exhaustive treatment of snake characters, but were for general comparison between taxa. Measurements were made to the nearest mm, and most samples consisted of only several individuals per species; in genera with a large number of species, the samples were, of course, larger. Thus, the ranges of variation presented in the tables are not likely to represent the entire variation present in the natural population of each genus. Those data were intended to indicate whether, for example, a snake had a large, moderate, or small eye in comparison with other taxa.

A black and white reproduction of a color slide does not exhibit as much detail as does the original. However, the reproductions in the journal *Cryptozology* are remarkably good. The original slides, as clearly stated in our paper, are deposited in the Herpetology Department of the Museum of Comparative Zoology (MCZ) at Harvard University (K948-950). Pauwels and Meitre could have borrowed the type material in order to examine it. The original slides would have illustrated to them that what we presented in our paper was based on fact.

In response to Pauwels and Meitre, the reason we did not mention a possible relationship of *Cryptophidion* with *Xenopeltis* is because there is none. *Xenopeltis* was excluded from consideration with good reason (see p. 15 of our paper). VW is quite familiar with *Xenopeltis*. He has both observed live *Xenopeltis unicolor* in captivity, and dissected laboratory specimens. We recently collected data on an additional 12 specimens from *Xenopeltis unicolor* from India, Myanmar (Burma), Thailand, Sumatra, Borneo, and the Philippines in the MCZ collection to compare with *Cryptophidion*. The data presented in Table 1 (this Response) are taken from those specimens, plus other dissected material (CAS 15801; MCZ 3094-95, 3114, 4223, 5483, 13310, 14697, 20341-43, 25603, 173225; FMNH 180278), ranging in size from 371-1,123 mm, and including both males and females.

Xenopeltis unicolor has been known to the scientific community for nearly 170 years, having been described by Carl Reinwardt in Friedrich Boie (1827, *Bermerkungen über Merrem's Versuch eines Systems der Amphibien, Isis von Oken*, Vol. 20 [6]: 564). A summary of published literature and personal observations reveals that it is a short-tailed, cylindrical-bodied, iridescent snake with a depressed snout and small eye. It is bicolored, with a dark brown or purple dorsum. The upper lip and entire ventral surface is white, milky-white, or yellowish, with the subcaudals sometimes exhibiting brown margins along their anterior edges (with increasing pigmentation posteriorly). The lowermost two to three scale rows are widely bordered in white, sometimes presenting the appearance of ventrolateral stripes. The first or paraventral scale row, which is greatly enlarged as in all booids, is usually entirely

white like the venter. Some individuals have brown centers to the supralabials, infralabials, or costals of row I. Only rarely has the venter been described as pink or pale-tan, the subcaudals as brown, or the upper lip as dark.

The major works describing or illustrating *Xenopeltis unicolor* include Hermann Schlegel (1837, *Essai sur la Physionomie des Serpents, Partie Descriptive*, Arnz, Leiden, and 1839, *Abbildungen neuer oder Unvollständig Bekannter Amphibien, nach der Natur oder dem Leben entworfen Herausgegeben und mit einem Erläuternden Texte begleitet*, Arnz, Düsseldorf, 1837-1844); Albert C. L. G. Günther (1864, *The Reptiles of British India*, Ray Society, London); Georges Jan and Ferdinand Sordelli (1865, *Iconographie Générale des Ophiidiens, Tome Premier [livrais. 1 à 17]*, J. B. Baillière, Paris, 1860-1866, Pls. 1-102); William Theobald (1868, *Catalogue of the Reptiles of British Burma, Embracing the Provinces of Pegu, Martaban, and Tenasserim; With Descriptions of New or Little-Known Species*, *Journal of the Linnean Society, Zoology*, Vol. 10 [41]: 4-67, and 1876, *Descriptive Catalogue of the Reptiles of British India*, Thacker, Spink, Calcutta); Edward Nicholson (1870, *Indian Snake: An Elementary Treatise on Ophiology with a Descriptive Catalogue of the Snakes Found in India and the Adjoining Countries*, Higginbotham, Madras); George A. Boulenger (1890, *The Fauna of British India, Including Ceylon and Burma. Reptilia and Batrachia*, Taylor and Francis, London, 1893, *Catalogue of the Snakes in the British Museum [Natural History]*, Vol. I. British Museum [Natural History], London, and 1912, *A Vertebrate Fauna of the Malay Peninsula, from the Isthmus of Kra to Singapore, Including the Adjacent Islands. Reptilia and Batrachia*, Taylor and Francis, London); Stanley F. Flower (1899, *Notes on a Second Collection of Reptiles Made in the Malay Peninsula and Siam, from November 1896 to September 1898, with a List of Species Recorded from Those Countries*, *Proceedings of the Zoological Society of London*, Vol. 1899: 600-96); Frank Wall (1909, *A Popular Treatise on the Common Indian Snakes. Part X. The Iridescent Earth Snake [Xenopeltis unicolor]*, *Journal of the Bombay Natural History Society*, Vol. 19 [2]: 287-98); Malcolm A. Smith (1914, *The Snakes of Bangkok*, *Journal of the Natural History Society of Siam*, Vol. 1 [1]: 5-18, and 1943, *The Fauna of British India, Ceylon and Burma, Including the Whole of the Indo-Chinese Sub-Region. Reptilia and Amphibia. Vol. III.-Serpentes*, Taylor and Francis, London); Nelly de Rooij (1917, *The Reptiles of the Indo-Australian Archipelago, II. Ophidia*, E. J. Brill, Leiden); Edward H. Taylor (1918, *Reptiles of Sulu Archipelago*, *Philippine Journal of Science*, Vol. 13D [5]: 233-67, 1922, *The Snakes of the Philippine Islands*, National Institute of Science and Technology, Manila, and 1965, *The Serpents of Thailand and Adjacent Waters*, *University of Kansas Science Bulletin*, Vol. 45 [9]: 609-1096); Doris M. Cochran (1930, *The Herpetological Collections*

made by Dr. Hugh M. Smith in Siam from 1923 to 1929, *Proceedings of the United States National Museum*, Vol. 77 [11]: 1-39); Clifford H. Pope (1935, *Natural History of Central Asia, Vol. X. The Reptiles of China: Turtles, Crocodilians, Snakes, Lizards*, American Museum of Natural History, New York); René Bourret (1936, *Les Serpents de l'Indochine, Tome II, Catalogue Systématique Descriptif*, Henri Basuyau, Toulouse); Robert Mertens (1943, *Systematische und ökologische Bemerkungen über die Regenbogenschlange, Xenopeltis unicolor Reinwardt*, *Der Zoologische Garten [N.F.]*, Vol. 15 [5-6]: 213-20); R. A. M. Bergman (1955, *The Anatomy of Xenopeltis unicolor*, *Zoologische Mededelingen*, Vol. 33 [22]: 209-25); J. K. P. van Hoesel (1959, *Ophidia Javanica*, Pertjetakan Archipel, Bogor, Indonesia); Alan E. Leviton (1959, *Systematics and Zoogeography of Philippine Snakes*, Unpublished Doctoral Dissertation, Stanford University, Stanford, and 1983, *Contribution to a Review of Philippine Snakes, XIV. The Snakes of the Genera Xenopeltis, Zaocys, Psammodynastes and Myersophis*, *Philippine Journal of Science*, Vol. 112 [3-4]: 195-223); Simon Campden-Main (1970, *A Field Guide to the Snakes of South Vietnam*, Smithsonian Institution, Washington); J. Deuve (1970, *Serpents du Laos*, Office de la Recherche Scientifique et Technique Outre-Mer, Paris); Hubert Saint-Girons (1972, *Les Serpents du Cambodge, Mémoires. Muséum National d'Histoire Naturelle, Zoologie*, Vol. 74A: 1-170); Bu-Ching Hu, Mai-Hui Huang, Zhan-Tai Xie, Er-Mi Zhao, Yao-Ming Jiang, Z. Yu, and J. Ma (1980, *Atlas of the Snakes of China*, Science and Technology Press, Shanghai, [in Chinese]); Beni C. Mahendra (1980, *Handbook of the Snakes of India, Ceylon, Burma, Bangladesh, and Pakistan*, *Annals of Zoology*, Vol. 22: 1-412); M. W. F. Tweedie (1983, *The Snakes of Malaya*, Singapore National Printers, Singapore); Angel C. Alcalá (1986, *Guide to Philippine Flora and Fauna, Vol. 10, Amphibians and Reptiles*, University of the Philippines, Manila); T. S. N. Murthy (1986, *The Snake Book of India*, International Book Distributors, Dehra Dun, India); Wan-Shu Tian, Yao-Ming Jiang, Gao Wu, Q.-X. Hu, Er-Mi Zhao, and Chin-Yuan Huang (1986, *Handbook of Chinese Amphibia and Reptilia*, Kolsuo-Choppa, Beijing, [in Chinese]); Frank F. Reitter and Jerry K. S. Lee (1978, *Common Snakes of South East Asia and Hong Kong*, Heinemann, Hong Kong); Francis L. K. Lim and Monty T.-M. Lee (1989, *Fascinating Snakes of Southeast Asia: An Introduction*, Tropical Press, Kuala Lumpur); Merel J. Cox (1991, *The Snakes of Thailand and their Husbandry*, Krieger Publishing, Malabar, Florida); and Er-Mi Zhao and Kraig Adler (1993, *Herpetology of China*, Society for the Study of Amphibians and Reptiles, Oxford, Ohio).

In addition, as stated above, VW has studied both living and preserved individuals of *Xenopeltis*. We will comment first on Pauwels and Meitre's eight points of contention from Table 2 of our paper, indicating our agree-

ment or disagreement with their arguments, and then discuss their allegations about our research methods. While their assertions regarding lack of differences between *Cryptophidion* and *Xenopeltis* in some of the characters from Table 2 are correct, for the majority of characters they are incorrect. After examining additional material, we stand corrected on the following two characters:

Character One (ventral proportion): Pauwels and Meitre are correct in stating that the proportional ventral width is not significantly different between *Cryptophidion* and *Xenopeltis*. The values for *Xenopeltis* should read "1" and not "0" in Table 2 of our paper.

Character Four (temporolabial scale): Pauwels and Meitre are technically correct with respect to this character, as *Xenopeltis* does possess a temporolabial shield by definition. However, this is a convergent feature, and not homologous with that of the Elapidae. The "temporolabial" of *Xenopeltis* is of similar size and shape to the other temporals, the parietals, and the azygous occipital on the dorsum of the head, so it can be considered a primitive scutellation pattern in view of the basal phylogenetic position of *Xenopeltis*. The temporolabial shield of the Elapidae has been cut from the large penultimate supralabial, as found in the cobra *Naja melanoleuca* (Samuel B. McDowell, 1967, *Aspidomorphus*, a Genus of New Guinea Snakes of the Family Elapidae, with Notes on Related Genera, *Journal of Zoology*, Vol. 151: 497-543), and is thus a derived feature of elapid snakes.

Character Two (anal shield): This character is not demonstrable as being either the same or different in *Cryptophidion* and *Xenopeltis*. It is not clear in the original color slide of the ventral surface (Fig. 3 of our paper) whether the anal shield is divided or single, although it appears undivided. Following the enlarged preanal shield discussed by Pauwels and Meitre are what appear to be two undivided shields: the anal and the first subcaudal. However, the resolution of detail in the cloacal region is the poorest area of the slide; nothing definitive can be concluded. This character is ambiguous.

Pauwels and Meitre are incorrect in their assertions that the following five characters do not differ between *Cryptophidion* and *Xenopeltis*.

Character Three (infralabials): A reconstruction of the chin drawn from the projected and enlarged image of the original color slide (K950) shows the first four infralabials contacting the pregenials (Fig. 1, this Response). The first pair of infralabials, which are twice as long as broad, meet on the midline and separate the genials from the mental. The second and third pairs are roughly as broad as long, and half the size of the fourth pair. The fifth pair is smaller than the fourth, but larger than either the second or third. The sixth, seventh, and eighth are successively reduced in size, with the sixth smaller than the second.

Character Five (rostral profile): The snout of *Cryptophidion* is pointed,

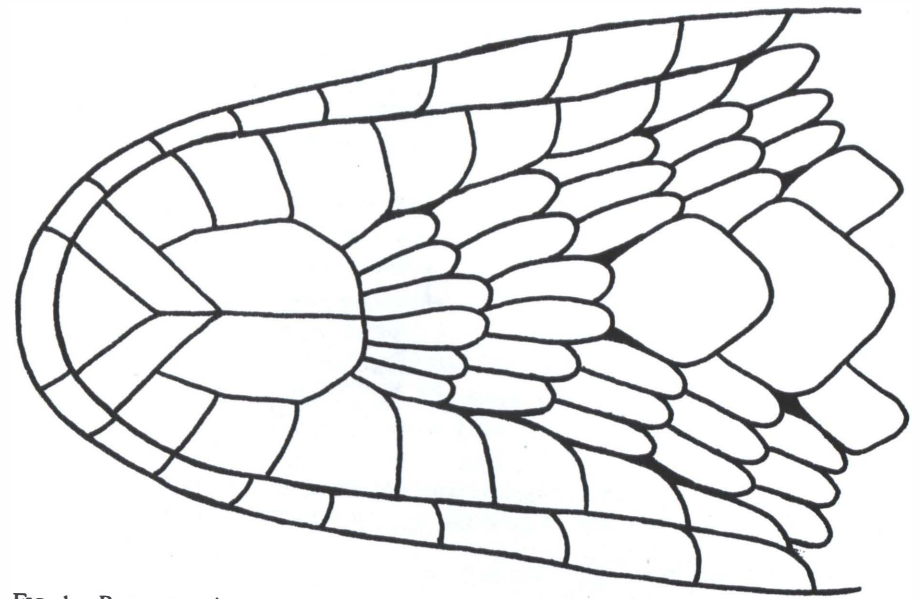


FIG. 1.—Reconstruction by the authors of ventral view of chin of *Cryptophidion* (based on existing color slide).

while that of *Xenopeltis* is blunt. The acuteness of the rostral tip is more evident in the original color slides than in the reproductions in this journal. There is no doubt that the lateral snout profile of *Cryptophidion* is acutely pointed, while that of *Xenopeltis* is blunt or (at most) obtusely pointed (Figs. 2a-c, this Response).

Character Six (vertical eye proportion): There are several different ways to measure this character, and, as seen from the *Cryptophidion* illustrations, changing the viewing angle changes the values (as noted by Pauwels and Meitre). We believe that the traditional method of measuring horizontal eye size in relation to its distance to the tip of the snout is an index that more clearly describes the proportional length of the snout. We used vertical eye diameter in relation to maximum head depth (including lower jaw), as we think it better describes eye size in relation to head size. The ratios derived by Pauwels and Meitre for vertical eye proportion in 21 specimens of *Xenopeltis* (0.19-0.41) range above and below that for the photograph (Fig. 2 of our paper) of *Cryptophidion* (0.33), which best represents the snout region. Data from our 13 additional specimens of *Xenopeltis* (Table 1, this Response) ranged from 0.17-0.27 ($\bar{x}=0.21$). While the difference is not as great as we originally suggested (i.e., 0.4 vs. <0.33), the vertical eye proportion is larger in *Cryptophidion* than in *Xenopeltis*.

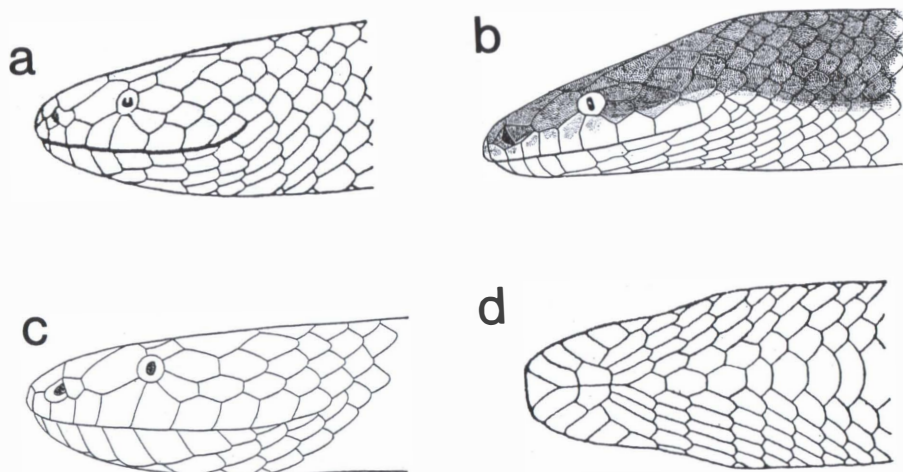


FIG. 2a.—Lateral view of the head of *Xenopeltis unicolor* (from Hu et al., 1980: Fig. 10)

FIG. 2b.—Lateral view of the head of *Xenopeltis unicolor* depicting typical coloration (from Smith, 1943: Fig. 31)

FIG. 2c.—Lateral view of the head of *Xenopeltis unicolor* (from Boulenger, 1890: Fig. 85, and 1912: Fig. 36)

FIG. 2d.—Ventral view of the head of *Xenopeltis unicolor* (from Hu et al., 1980: Fig. 10)

Character Seven (horizontal eye proportion): The ratios obtained from Figs. 1 and 2 of our paper are different for relative (horizontal) eye size due to the two different angles at which the specimen was photographed (as is obvious from the different obtainable measurements and proportions). Neither view appears to be a directly perpendicular shot; the specimen's head in both is oblique, with its snout oriented slightly toward the camera. Due to this foreshortening, the horizontal length of the snout is actually greater than it appears in the photograph. Pauwels and Meitre's *Xenopeltis* sample ($n=21$) had values ranging from 2.9-5.4 for horizontal eye proportion, as compared with our original data (including measurements from illustrations in the literature) which ranged from 3.3-6.4. Our *Xenopeltis* sample ($n=13$) has a range of values from 3.0-4.9 ($\bar{x}=4.1$) for this character, while the value for *Cryptophidion* from Fig. 2 of our paper is 3.1 (see Table 1, this Response). Thus, *Cryptophidion* has a shorter snout and larger relative eye size than does *Xenopeltis*. This is readily apparent when comparing our illustrations with photographs such as those in van Hoesel (1959, above: Fig. 12); Mertens (1943, above: Fig. 2); and Saint-Girons (1972, above: Pl. 5); or illustrations such as in Schlegel (1837, above: Pl. 1, Fig. 9); Soulenger (1890, above: Fig. 85); this Response (Fig. 2c); Wall (1909, above: Pl. 10, Fig. 5, diagram I); Taylor (1922, above: Fig. 5); Pope (1935, above: Pl. 5, Figs. H-J); Bourret (1936, above: Fig. 12); Deuve (1970, above: Pl. 3, Fig. 2a); Reitingger and

Lee (1978, above: 104); Hu et al. (1980, above: Figs. 9-10, Pls. 2-3); this Response (Fig. 2a); and Mahendra (1980, above: Fig. 24).

Character Eight (ventral coloration): In all of the examined specimens of *Xenopeltis*, the venter is immaculate creamy-white or white, the first (paraventral) scale row is white (in larger specimens an anterior brown edge occurs on each scale), and the upper lip is white. The subcaudals are either entirely white or possess a brown margin to each scale. Small specimens (less than 550 mm snout-vent length [SVL]) have the paraventral scale row entirely white, but in larger individuals (greater than 600 mm SVL) the percentage of area of brown bordering the anterior edge of the scale varies from 0.20-0.33, with a mean of 0.25 ($n=7$). Light coloration of the venter has been described and illustrated by Schlegel (1837, above: 21, Pl. 1, Figs. 9-10); Schlegel (1844, above: Pl. 35); Günther (1864, above: 181); Jan and Sordelli (1865 in 1860-1866, above: Livr. 9, Pl. 5); Theobald (1868, above: 37); Theobald (1876, above: 140); Boulenger (1893, above: 168); Flower (1899, above: 657); Wall (1909, above: 293, Pl. 10, Figs. 5-6); Smith (1914, above: 12-13); Rooij (1917, above: 40, Figs. 20-21); Mertens (1943, above: 214-15, Figs. 2-3); Smith (1943, above: 102, Fig. 31); Bergman (1955, above: 210); Leviton (1959, above: 23-4); Taylor (1965, above: 662); Campden-Main (1970, above: 4); Deuve (1970, above: 72, Pl. 3, Figs. 2d-e); Saint-Girons (1972, above: 37); Hampton W. Parker and Alice G. C. Grandison (1977, *Snakes: A Natural History*, British Museum [Natural History], London, Pl. 2 [top]); Wolf-Eberhard Engelmann and Fritz J. Obst (1981, *Snakes: Biology, Behavior and Relationship to Man*, Exeter Books, New York: 143); Leviton (1983, above: 199); Tweedie (1983, above: Pl. 1); Murthy (1986, above: 13); Tian et al. (1986, above: Pl. 12, Fig. 36); John M. Mehrtens (1987, *Living Snakes of the World in Color*, Sterling Publishing, New York: 15); Francis L. K. Lim and Monty T.-M. Lee (1989, *Fascinating Snakes of Southeast Asia: An Introduction*, Tropical Press, Kuala Lumpur: 33); John Coborn (1991, *The Atlas of Snakes of the World*, T.F.H. Publications, Neptune City, New Jersey: 175); Cox (1991, above: 101, Pls. 8-9); Kelvin K. P. Lim and Francis L. K. Lim (1992, *A Guide to the Amphibians and Reptiles of Singapore*, Singapore Science Center, Singapore: 50); and Zhao and Adler (1993, above: Pl. 31).

Only two exceptions are known to a uniformly light venter in *Xenopeltis unicolor*: a specimen from Java reported by L. D. Brongersma (1930, *Abnormal Coloration of Xenopeltis unicolor* Reinw. [1827], *Copeia*, 1930 [3]: 87) that is nearly black in color, and a totally melanistic individual from Sumatra reported by Mertens (1943, above: 217) that is black both above and below. However, even though it is melanistic, the light borders around the lower scale rows are still evident in the Javan specimen. The abnormal aspect of these two aberrant individuals is that the venter is quite black, a condition not seen in *Cryptophidion*, which, while dark, is certainly not black.

Due to an error on the part of VW, the key for "color of venter" in Tables 2-5 of our paper was transposed between "dark" and "light." The key for dark venter should read "1," and that for light venter "2." Pauwels and Meitre may have only examined the tables, as our description of *Cryptophidion annamense* (p. 12) clearly states: "The venter appears immaculate bluish-gray. The anterior base of each of the subcaudals has a black border. . ." Fig. 3 of our paper shows the ventral surface of *Cryptophidion* to be uniformly dark. Pauwels and Meitre argue against themselves on character eight by stating that *Xenopeltis* does indeed have a light venter.

Thus, of the eight characters discussed by Pauwels and Meitre, two (#1, 4) are justified, one (#2) is ambiguous, and five (#3, 5, 6, 7, 8) are unjustified. In conjunction with the latter five features (infralabial number, lateral rostral profile, vertical eye proportion, horizontal eye proportion, and ventral coloration) are an additional 14 distinctions between *Cryptophidion* and *Xenopeltis*.

1) In lateral profile, the lower jaw (and gular region) of *Xenopeltis* is prominent, and consists of up to $\frac{1}{3}$ of the head depth (Boulenger, 1912, above: Fig. 36; Hu et al., 1980, above: Fig. 10; this Response, Fig. 2a), a situation not seen in most colubrids, burrowers, or *Cryptophidion* (Figs. 1-2 of our paper).

2) The upper lip and chin of *Xenopeltis* are always light in coloration, like the venter (Fig. 2b, this Response), whereas in *Cryptophidion* the entire head is uniformly dark (Figs. 1-2 of our paper).

3) Comparison of the vertical diameter of the eye to the height to the supralabial beneath it is a third method of measuring relative eye size. Data on the 12 *Xenopeltis* at hand for vertical eye diameter/supralabial height range from 0.50-0.75 ($\bar{x}=0.66$), while in *Cryptophidion* (Fig. 2 of our paper) the ratio is 0.40. The values of *Cryptophidion* again fall outside the range of those for *Xenopeltis*. What this means is that the eye is "moderate" in *Cryptophidion*, but "small" in *Xenopeltis unicolor*. This is so reported in all descriptions of the species in the literature as cited above.

4) The cross-section of the cylindrical body in *Xenopeltis* is round, while that of *Cryptophidion* is more flattened ventrally, as in many colubrids. This is one of those gestalt images that is learned through extensive experience with a diverse assemblage of snakes. Booids and colubroids have different body plans that are obvious and recognizable.

5) The gestalt of the ventral view of *Cryptophidion* is of a small-sized burrowing snake less than 300 mm long, a view supported by the suite of morphological characters that are correlated with subterranean life in snakes. Juvenile *Xenopeltis* exhibit a striking color pattern that consists of a light nuchal collar around the neck and posterior head (Theobald, 1868, above: 37; Wall, 1909, above: 293; Rooij, 1917, above: Fig. 21; Alcalá, 1986, above:

Fig. 128; Cox, 1991, above: Pl. 9), yet there is no indication of a light collar in *Cryptophidion*.

6) In ventral view, the tip of the snout is broadly rounded in *Cryptophidion* (Fig. 3 of our paper; Fig. 1, this Response) but tapered in *Xenopeltis* (Saint-Girons, 1972, above: Pl. 5).

7) The pregenials of *Xenopeltis* are wider anteriorly than posteriorly, and roughly triangular in shape (Fig. 2d, this Response); in *Cryptophidion*, they are not significantly wider anteriorly than posteriorly (Fig. 1, this Response).

8) In *Cryptophidion*, there are two small pairs of postgenials lying along the midventral line between the pregenials and the first gular (Fig. 1, this Response). *Xenopeltis* lacks any postgenials oriented along the midline; the pair of scales caudad of the pregenials are obliquely oriented in an inverted "V" shape (Fig. 2d, this Response).

9) The scales between the genials and the first ventral shield in *Xenopeltis* make a gradual transition in size and shape so that it is difficult to objectively determine which shield represents the first ventral (Fig. 2d, this Response). Employing the definition of "about as broad as deep" for the gulars, all shields that are distinctly wider than deep, but narrower in width than the ventral series, are termed the gular-ventral transitional (GVT) shields. Preventral scales consist of gulars and GVT scales. In 12 *Xenopeltis*, the gulars range from 5-7 ($\bar{x}=5.4$), the GVT shields from 2-4 ($\bar{x}=2.75$), and the mean number of preventrals from 7-11 ($\bar{x}=8.15$). *Cryptophidion* exhibits 5 gulars, but lacks any GVT shields, as there is an abrupt transition to the first ventral, which is more than twice as wide as the fifth gular.

10) The tapered head of *Xenopeltis* is not distinct from the neck, but joins the body without interruption in the nuchal area (Wall, 1909, above: 292; this Response, Fig. 2d). In *Cryptophidion*, a weak but distinct neck constriction is present, as the head is wider than the neck (Fig. 3 of our paper).

11) The shape of the ventrals is different in the two taxa. The ventral shields in *Xenopeltis* are straight and transverse, as in all booids, while in *Cryptophidion*, although they are transverse, they are slightly convex posteriorly, as in many colubrid snakes.

12) The relative size of the paraventral or first scale row compared to the ventrals is different in *Cryptophidion* and *Xenopeltis*. The transverse width of the paraventral row in *Xenopeltis* is larger, in comparison with ventral width, than in *Cryptophidion*. In *Xenopeltis*, mean values of ventral width/paraventral width, measured in the neck region, at midbody, and in the cloacal region, are 2.0, 2.1 and 2.1, respectively, while in *Cryptophidion* they are 2.3, 2.7 and 2.3 (see Table 1, this Response). The grand mean of all measurements is 2.1 for *Xenopeltis* ($n=36$), and 2.4 for *Cryptophidion* ($n=3$).

13) The gestalt of the ventral view of the tail in *Cryptophidion* resembles that of colubrids, while in *Xenopeltis* is more booid. The tapering of the

TABLE 1.—Data on *Xenopeltis unicolor* in the collection of the Museum of Comparative Zoology, Harvard University.

Cat no.	Country	LOA	TL	MTD	TP	VE	VH	VEP	HE	S	S/E	SL
C. a.	Vietnam	—	—	—	16.0	—	—	0.33	—	—	3.10	—
13310	Thailand	371	44	4.5	9.8	1.00	5.0	0.20	1.50	5.75	3.80	1.50
5483	Sumatra	458	47	5.0	9.4	1.50	6.5	0.23	2.00	6.00	3.00	2.00
14697	Borneo	603	68	6.0	11.3	1.50	7.0	0.21	1.90	7.50	3.90	3.00
25603	Philippines	623	72	7.0	10.3	1.50	7.0	0.21	1.75	8.50	4.90	2.50
20342	Thailand	668	66	8.5	7.8	1.50	9.0	0.17	2.50	10.50	4.20	3.00
4223	Myanmar	755	72	7.5	9.6	2.00	10.0	0.20	2.50	10.00	4.00	3.00
20341	Thailand	780	78	8.0	9.8	2.00	9.0	0.22	2.00	9.50	4.75	3.00
20343	Thailand	803	73	7.5	9.7	2.00	8.5	0.24	2.33	9.50	4.10	2.75
3094	Thailand	745*	?	?	?	3.00	12.0	0.25	3.00	11.50	3.80	4.00
3114	India	900	81	9.0	9.0	3.00	15.0	0.20	3.00	12.00	4.00	4.00
3095	Thailand	995	91	11.5	7.9	2.75	14.0	0.20	3.00	13.50	4.50	4.50
173225	Unknown	1123	81	10.0	8.1	3.00	11.0	0.27	3.00	14.50	4.80	4.50

Keys to abbreviations in Table 1: Cat no. (MCZ catalogue number), LOA = total length in mm (* = truncated tail, value = snout-vent length), TL = tail length, MTD = midtail diameter, TP = tail length/midtail diameter, VE = vertical eye diameter, VH = vertical head depth, VEP = vertical eye ratio, HE = horizontal eye diameter, S = snout length, S/E = horizontal eye ratio, SL = subocular supralabial height, E/SL = eye/supralabial ratio, G = gulars, GVT = gular-ventral transitional scales, D = dorsum coloration (B = brown, B-G = bluish gray, P = purple, P-B = purplish brown), V = ventral coloration (B-G = bluish gray, W = white, Y-W = yellowish-white), AV = anterior ventral (V) width, APV = anterior paraventral (PV) width, AR = anterior V/PV width ratio, MV = midbody ventral width, MPV = midbody paraventral width, MR = midbody V/PV width ratio, PV = posterior ventral width, PPV = posterior paraventral width, PR = posterior V/PV width ratio, \bar{x} = mean V/PV ratio.

tail is stronger in *Cryptophidion*, and less abrupt in *Xenopeltis*. This is apparent in the ratio of tail length/midtail diameter. Data on 11 *Xenopeltis* reveal a range of 7.8–11.3 (\bar{x} =9.3), while the value for *Cryptophidion* is 15.0 (see Table 1, this Response). The tail of *Cryptophidion* is more strongly tapered than that of *Xenopeltis*, which is consistent with the body form of colubrids vs. booids.

14) The terminal spine in *Cryptophidion* is elongate and thin (twice the length of the terminal subcaudals), while that of *Xenopeltis* is short, thick, and conical (barely longer than a single subcaudal).

Even if *Cryptophidion* and *Xenopeltis* were similar for all 26 characters presented in Table 2 of our paper, that still would not demonstrate that they were the same genus or species. There is a similarity in scale counts and morphological proportions of *Cryptophidion* and *Xenopeltis* (as noted in our paper), but we maintain, and have once again demonstrated, that they are not the same animal. We suggest that the similarities in these two taxa are due to a convergent, burrowing lifestyle, as detailed in our paper (p. 12).

Concerning the Code, we are well versed in its application. In reference to the type of *Cryptophidion*, we clearly stated (p. 5) that the holotype was the specimen that was illustrated, and we referred only informally to the slides as iconotypes, because that is what they are, regardless of whether the International Commission on Zoological Nomenclature recognizes that cat-

TABLE 1.—Extended.

Cat. no.	E/SL	G	GVT	D	V	AV	APV	AR	MV	MPV	MR	PV	PPV	PR	\bar{x}
C. a.	0.40	5	0	B-G	B-G	—	—	2.3	—	—	2.7	—	—	2.3	2.4
13310	0.67	6	2	B	W	5.0	3.0	1.7	7.0	3.5	2.0	5.5	3.0	1.8	1.8
5483	0.75	5	2	B	Y-W	6.0	3.5	1.7	8.5	4.0	2.1	6.5	3.5	1.9	1.9
14697	0.50	5	3	B	W	10.0	5.0	2.0	11.0	5.5	2.0	8.5	3.5	2.4	2.1
25603	0.60	7	4	B	W	9.5	4.0	2.4	12.0	5.0	2.4	10.0	4.0	2.5	2.4
20342	0.50	5	3	B	Y-W	12.0	7.0	1.7	14.5	7.0	2.1	10.5	5.0	2.1	2.0
4223	0.67	6	2	B	Y-W	10.0	4.5	2.2	12.5	6.0	2.1	13.5	7.0	2.0	2.1
20341	0.67	6	4	B	Y-W	11.5	5.5	2.1	14.0	7.0	2.0	10.5	4.5	2.3	2.1
20343	0.73	5	3	B	Y-W	13.0	5.5	2.4	14.0	6.5	2.2	10.0	4.5	2.2	2.3
3094	0.75	4	3	P	Y-W	12.5	6.5	1.9	19.5	9.5	2.1	15.5	6.5	2.4	2.1
3114	0.75	5	3	P-B	Y-W	12.0	7.0	1.7	16.5	9.0	1.8	13.5	7.0	1.9	1.8
3095	0.61	6	2	B	Y-W	16.0	8.5	1.9	22.0	11.0	2.0	18.5	9.5	1.9	1.9
173225	0.67	5	2	B	W	13.5	7.5	1.8	19.5	8.5	2.3	14.0	6.0	2.3	2.1

egory as official or not. Referring to the slides as iconotypes merely facilitated the discussion. Pauwels and Meitre's method of attempting to synonymize a name without first examining the type material is unacceptable.

In conclusion, based upon the ample evidence in the original description and that presented above, *Cryptophidion* should continue to be recognized as a valid taxon. Incidentally, the valid date for the name *Cryptophidion annamense* Wallach and Jones is 1994, not 1992. Volume 11 of *Cryptozoology* was for the calendar year 1992, and it would normally have been published in that year. However, publication did not occur until April, 1994 (a date which appears on the inside cover of the journal). Consequently, according to Article 21(b), V, of the Code, the name becomes available only in 1994.

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ARE LAKE MONSTER SIGHTING TIMES BIOLOGICALLY OR CULTURALLY BASED?

(Comment on Yasushi Kojo, 1992, Distributional Patterns of Cryptid Eyewitness Reports from Lake Champlain, Loch Ness, and Okanagan Lake, *Cryptozoology*, Vol. 11: 83-89)

Kojo has compared the sighting times of the large unknown animals reported by eyewitnesses in Lake Champlain (eastern U.S.A.), Loch Ness (Scotland), and Okanagan Lake (western Canada). The distribution in time of sightings at Loch Ness and Okanagan Lake are quite similar, if not identical, while those at Lake Champlain do not show the bimodal distribution if sightings reported at the other two lakes.

He concludes that these differences reflect the behavior of the unknown animals in question rather than the behavior of the observers. He maintains that it is unlikely that the midday behavior of the observers at Lake Champlain is significantly different from the midday behavior of observers at the other two lakes.

To the contrary, differences in midday sightings probably do reflect human sociocultural-based behaviors. One example would be the taking of a midday *siesta*, when sightings are far less likely to be made. The sociocultural similarities of observers in Scotland and Canada in this regard would explain the similar reporting patterns at Loch Ness and Okanagan Lake, as well as the absence of a midday dip in sighting reports at Lake Champlain, where observers are predominantly American, with a different cultural heritage.

Furthermore, Lake Champlain is comparatively large (over 100 miles in length), resulting in many observations being made from boats. Consequently, observers remain in a position to have sightings even during on-board *siestas*.

Of course, the groups of observers at the three lakes are far from homogeneous, representing neither pure samples of *siesta*-takers or non-*siesta*-takers, so that these surmises are suggestive at best.

Clearly, what is needed to clarify these questions is a study as precise as possible of the observers rather than the reported animals.

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(Roy Mackal is a biologist who has studied the evidence for unknown animals in Loch Ness and Lake Champlain for many years. He is the author of the 1976 book *The Monsters of Loch Ness*, and serves as Vice President of the International Society of Cryptozoology.)

RESOLVING THE PROBLEM

(Response to Mackal)

When I visited Loch Ness some years ago, I asked some local people if the custom of a midday *siesta* is prevalent there. The answers were "no." I would thus like to know the basis for Mackal's assertion as to the prevalence of this custom at Loch Ness and Okanagan Lake. I would also appreciate third party opinions in this forum from anybody who is familiar with local customs in these areas.

Mackal states that the difference in distributional patterns of sighting times is due merely to the sociocultural difference between Americans and Scots/Canadians. In order to determine if this is so, it would be very helpful to examine distributional patterns of sighting times of similar animals reported in other lakes in the U.S., Scotland, and Canada. Table 1 and Fig. 1 show that of the animals reported in Lake Memphremagog, Quebec, Canada. The number of sighting times that I am currently aware of is not very large, and most of these reports came from the U.S. part of the lake. Nonetheless, the distributional pattern (Fig. 1) is very similar to that from Lake Champlain.

It is highly probable that sighting reports by Canadians and Americans would have an identical distribution pattern. In this connection, I wonder if Jacques Boisvert, a Canadian investigator (Jacques Boisvert, 1992, *The Sea Serpent of Lake Memphremagog*, Société Internationale de Dracontologie du Lac Memphremagog, Magog, Quebec), can provide this forum with sighting time data from the Canadian side of the lake.

In addition to determining the midday behavior of the local people, and examining the distributional patterns of sighting times of similar animals reported in other lakes, an examination of sighting times during systematic surveillance would be useful in resolving the problem. For instance, in June,

TABLE 1.—Sighting times of the large unidentified animals reported in Lake Memphremagog, Quebec, Canada.

Date	Time	Reference
Aug. 12, 1983	5:00 p.m.	Personal communication from B. Malloy
Jun. 15, 1986	7:30 p.m.	<i>The Chronicle</i> , July 16, 1986
May 1989	7:45 p.m.	Personal communication from B. Malloy
Jul. 1989	8:00 p.m.	Personal communication from B. Malloy
Sep. 1989	3:30 p.m.	Personal communication from B. Malloy
May 15, 1991	7:30 p.m.	Personal communication from B. Malloy
Sep. 13, 1991	2:00 p.m.	Boisvert 1992:3 (see text)
Aug. 7, 1992	4:00 p.m.	<i>The Newport Daily Express</i> , August 24, 1992
Oct. 1992	6:15 p.m.	<i>The Newport Daily Express</i> , October 21, 1992
Aug. 2, 1992	6 or 7 a.m.	<i>The Toronto Star</i> , May 1, 1993
Aug. 25, 1992	4-5 p.m.	Personal communication from B. Malloy

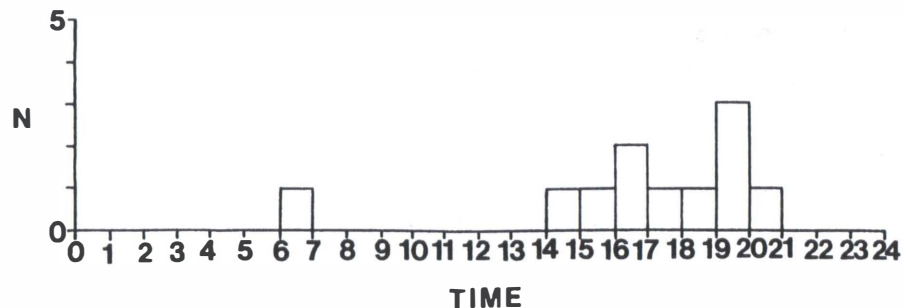


FIG. 1.—Sighting time distribution of the large unidentified animals reported in Lake Memphremagog, Quebec, Canada.

1963, the Loch Ness Investigation Bureau conducted systematic surveillance from 4 a.m. to 10 p.m. every day for two weeks, and it reported a sighting on June 13 (Nicholas Witchell, 1991, *The Loch Ness Story*, Revised and Updated Edition, The Guernsey Press, Guernsey: 118–9). Unfortunately, Witchell (1991, above) does not give the time of this sighting. Nonetheless, if it took place between 9 a.m. and noon, or between 2 and 5 p.m., the likelihood that the distributional pattern of sighting times being due to the animal's own behavior is greatly enhanced. I would appreciate it if anybody who knows the sighting time of this report would provide the information to this forum.

With regard to the animals reported at Okanagan Lake, quite a few sightings have been made during systematic surveillance (John Kirk, 1989, BCCC Report on Okanagan Lake, 1989, *Cryptozoology*, Vol. 8: 75–9; 1990, BCSCC Report on Okanagan Lake, 1990, *Cryptozoology*, Vol. 9: 85–7). All of these sightings occurred between 10:45 and 11:30 a.m., or between 2:15 and 3:55 p.m. Unfortunately, however, from what time to what time each surveillance was conducted is not reported. It should be stressed that reporting surveillance times is as important as reporting sighting times, especially if sightings took place during such surveillance. It would be helpful if Kirk could provide the information in this forum.

If we could have the cooperation of other investigators, we may well be able to solve the problem unequivocally.

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DYSFUNCTIONAL SCIENTISTS

(Comment on Aaron M. Bauer, 1992, Review of *Natural Mysteries: Monster Lizards, English Dragons, and Other Puzzling Animals*, *Cryptozoology*, Vol. 11: 116–19)

Aaron Bauer's treatment of *Natural Mysteries* is useless as a book review, but it will be useful to historians a century from now. They will take note of it when they look back at our times, and wonder how scientists were so dysfunctional in the presence of so many unresolved biological mysteries. Those historians will detect that scientists were both misinformed and uninformed as a result of failures such as Bauer's.

The only criticism with substance in Bauer's treatment pertains to the appearance and habits of the giant amphibians of the Ohio Valley. I would reply that the habits and the carriage of external gills for an amphibian of this size are yet to be determined.

Nowhere in the book do I suggest that animals have maintained themselves in the Great Swamps *in situ* over millions of years. Such a fiction exists only in Bauer's mind. I am accused of being inconsistent when discussing the amphibians because of differing appearances. In the book, I sought to resolve that very circumstance by suggesting that, like the axolotl, the animals are seen in two differing stages. Bauer repeatedly resorts to hyperbole, and has contrived other criticisms that can best be dispelled by reading the book.

The effect of such a treatment is to discourage his readers from ever consulting this book, despite a limp and incongruous recommendation at the end. I cannot take seriously this pretense of a review after what was obviously a careless reading of the book.

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APPLYING RIGOROUS ANALYSIS

(Response to Hall)

Apparently, Mark Hall's concept of a *useful* review is synonymous with a *favorable* review. Hall's comment (and his book, for that matter) is not consistent with a scientific approach to cryptozoology. I do not maintain that my interpretation of data collected or phenomena observed is correct,

but I do insist that all cryptozoological data must be open for critical analysis, and that theories in cryptozoology must be falsifiable.

Like many people, Hall seems to regard such scrutiny as the denial of the reality of "biological mysteries." In fact, it is only through this process that cryptozoology advances, or achieves respectability in the scientific community. The goal of scientific respectability may be anathema to those who regard cryptozoology as the last bastion of free spirits, but, nonetheless, I believe it is a worthy aim. For more than a decade, dozens of researchers in diverse disciplines have demonstrated in the pages of the journal *Cryptozoology* that natural mysteries are subject to rigorous analysis. Some mysteries prove to be frauds; others are real, but ultimately mundane; some are astounding and unexpected; and still others remain mysteries despite critical analysis.

There are undoubtedly more things in and on this earth than we know of, but, by the same token, not everything is possible, and only a small subset of what is possible is plausible. I am greatly interested in *natural* mysteries, but to place the data and their interpretation beyond critical analysis, as Hall would appear to prefer, moves them into the realm of *supernatural* mysteries, and out of the realm of cryptozoology.

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ONZA! The Hunt for a Legendary Cat by Neil B. Carmony

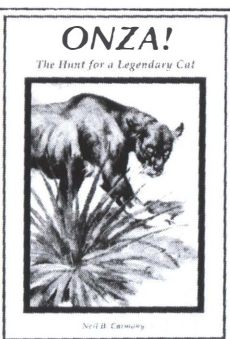
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These manuscripts should report on cryptozoological fieldwork. Only one manuscript copy need be submitted. Formal referencing should *not* be prepared, although references may be cited in parentheses *within* the text when appropriate (see examples in the journal); when so referencing, both the complete first (given) and second names (surnames) of authors being cited *should be spelled out*, and the names of journals, popular magazines, and books should also be *completely spelled out and unabbreviated*. Subheadings should be: *Introduction* (in which the type of cryptozoological event investigated is detailed, and the purpose or aims of the fieldwork, and the location and dates involved, are given; previous fieldwork by the author/s or others in the same geographic region should also be mentioned); *Narrative Description* (in which the field activities are described; e.g., lake surveillance, equipment used, interviews with aboriginal peoples); *Results* (in which the outcome of these efforts is described; the total lack of success of such fieldwork will not be used as a criterion in evaluating manuscripts); *Future Plans* (in which a brief discussion or statement on whether the author/s plan/s further fieldwork in the area is provided). Illustrations and maps are welcome, but original artwork and black and white photographs are required (color slides are not acceptable). All such material should be labeled as numbered "Figures" or "Tables," which must be cited in the text. On a separate sheet, the author/s should provide the figure and table captions. Manuscripts should be limited to a maximum of 1,500 words (longer manuscripts will be considered in special circumstances).

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Reviews of cryptozoological books or monographs, or books on scientific topics which have a direct bearing on cryptozoological matters, may be submitted unsolicited. It is recommended, however, that authors first contact the Editor to ensure that the book is not already being reviewed. Only one manuscript copy need be submitted. Manuscript length is left to the discretion of the author. Authors are requested to examine the journal for book review format.

Comments and Responses

Readers wishing to critique or comment on works previously published in the journal may do so. Comments not relating to works previously published in the journal will not be accepted. (Such comments may be submitted in the form of letters to *The ISC Newsletter*.) Authors wishing to respond to comments on their previously-published works may do so. The length of Comments and Responses is left to the discretion of the author/s, but may be shortened by the Editor if they are considered too lengthy. Two manuscript copies should be submitted. Formal referencing should *not* be prepared, although references may be cited in parentheses *within* the text when appropriate (see examples in the journal); when so referencing, both the complete first (given) and second names (surnames) of authors being cited *should be spelled out*, and the names of journals, popular magazines, and books should also be *completely spelled out and unabbreviated*. Comments and Responses should be titled, but subheadings should not be included. The name/s and address/es of the author/s should be followed by a line space and then a brief statement, italicized and in parentheses, describing specialties or interests e.g. (John Smith is a mammalogist with a special interest in the evolution of bats).

Referencing (Articles and Research Reports Only)

The last name/s of the author/s being cited, followed by the year of publication, should be placed in parentheses within the text itself. If the authors already are mentioned in the text, only the year need appear in parentheses. At the end of the manuscript, starting on a *new* page and under the capitalized heading of References Cited, the manuscript's formal references should be listed in alphabetical order by author/s (note that only references actually cited in the text should be listed). If several publications by the same author/s are cited, these should be listed in chronological order with the oldest publication appearing first. Both the authors' complete first (given) and second names (surnames) *should be spelled out*, and the year of the publication should be set off under the author's name. The names of journals, popular magazines, and books *should be completely spelled out and unabbreviated*. Volume numbers (in Arabic) and pages should be included (or dates in the case of newspapers and popular magazines), and book titles should be followed by the city of publication, a colon, and the name of the publisher.

Notes and footnotes should *not* be included. Authors should attempt to include such comments within the text itself. Contributors are urged to consult an issue of the journal and/or consult the Editor when in doubt.

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